THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XXXVIII.—*Observations on the Displacement of lines in the Solar Spectrum caused by the Sun's rotation;* by Professor C. A. YOUNG, of Dartmouth College.

[THE substance of this paper was read at the Buffalo Meeting of the American Association for the Advancement of Science, August 24th, 1876.]

Renewed interest in the question as to the effect of the motion of a luminous body in altering the wave length of the emitted light, has lately been excited by Van der Willigen's mathematical papers upon the subject, and the recent criticisms of Secchi upon the spectroscopic determinations of stellar motions published by Huggins, Vogel, Christie and others. The former, it will be recollected, impugns the received doctrine on mathematical grounds, and it must be acknowledged, that, although his reasoning is not admitted to be conclusive by most astronomers, it has produced a wide-spread distrust, which has been strengthened by the papers of Secchi. The objections of the latter seem however to have been fairly met by the reply of Mr. Christie, recently published in the Monthly Notices, showing the substantial agreement of the results obtained by the different observers after they had learned the delicate precautions essential to success in such observations.

Certainly it seems little short of self-evident that, whenever a point is communicating periodic vibrations to any medium whatever, and, by means of this medium, transmitting them to a second point at a distance, then the frequency, or virtual wave length, of these pulses received at the second point must

be affected by any relative motion of approach or recession between it and the source of the vibrations.

It is not difficult to verify this conclusion in the case of sound waves. The beautiful experiments of Vogel, lately published, show as the result of careful quantitative measurements, that the pitch of a locomotive whistle actually undergoes the precise alterations which theory requires, when the engine is either approaching the observer, or receding from him at a known velocity.

Undoubtedly a considerable point would be gained if we could obtain a similar verification in the case of light—if an alteration in the luminous pitch or wave length, produced in a ray of light by some known rate of motion, could actually be made sensible, measured, and shown to coincide with theory within observational limits of error. This verification unfortunately is not easy to obtain, because the velocity of light is so enormous that it is difficult to find an object sufficiently bright, and moving rapidly enough, to make the change of wave length perceptible in our instruments.

I think it was Zöllner who first suggested that the rotation of the sun might furnish the desired test, since its eastern and western limbs have a relative motion of nearly $2\frac{1}{3}$ English miles per second along the line of sight. But the displacement of lines in the spectrum due to this velocity is so small (in the case of the D lines about $\frac{1}{77}$ of the distance between them) that the dispersive power of the instruments heretofore employed by most observers, has been insufficient to make it clearly evident. Vogel alone (in 1871) seems to have succeeded in getting any measurements; his results for the sun's equatorial velocity of rotation ranging from 0·35 to 0·42 of a geographical mile, or from 1·62 to 1·94 English miles.

By using a diffraction grating, however, combined with a prism in such a way as to separate the overlapping spectra of the higher orders from each other, as described in my recent note* on the duplicity of the 1474 line, it is possible to obtain much greater dispersive power, and the displacement then becomes quite sensible.

The apparatus which I have employed consisted of a very fine diffraction grating of 8,640 lines to the inch (for which I am indebted to Mr. Rutherfurd) combined with a telescope and collimator each of $2\frac{1}{4}$ inches aperture and sixteen inches focal length, and a prism of 45° inserted between the grating and the object glass of the eye-telescope. The refracting edge of the prism was of course perpendicular to the lines on the grating. The grating, collimator, &c., were mounted on a wooden framework constructed for the purpose, and arranged to be

* This Journal, June, 1876.

attached to the 9½ inch Equatorial of the Dartmouth College Observatory, in place of its ordinary spectroscope. Undoubtedly a metallic mounting would have been firmer and better, but with careful manipulation the wooden arrangement answered reasonably well. The eye-telescope and collimator were at a fixed inclination, and the spectra of the different orders were brought into the field of view by turning the grating in the plane of dispersion. This of course made the dispersive power quite different for the spectra of the same order on opposite sides of the image of the slit. The eye-telescope magnified about twenty times and was provided with a micrometer* borrowed from one of the reading microscopes of the meridian circle.

Between the two D lines, the spectra of the sixth and eighth orders usually showed no less than eight other lines, most of which are supposed to be water lines, produced by the vapor in our atmosphere, and therefore of course not subject to displacement by the sun's rotation. I was in hopes to make use of them as reference points, and to determine the displacement of the D lines by simply measuring the intervals with the micrometer. I soon found, however, that the atmospheric lines were too faint and shadowy to admit of sufficient accuracy of bisection, especially by the rather coarse threads with which the micrometer had been provided for the purpose of observing in a feeble light. I was accordingly compelled to make the observations as follows—

The grating having been adjusted so as to bring the group of lines to be observed into the center of the field of view, the spectroscope was turned around the optical axis of the telescope until the slit was accurately north and south, so that, if placed tangent to the eastern limb of the sun, a motion of the Right Ascension tangent screw would bring the western limb to tangency. Of course proceeding in this manner the observations were made at points not precisely on the sun's equator, but having a solar latitude ranging from 2° to 15°, according to the date. It was thought better to do this, however, than to risk the disturbance which might be produced by using the Declination tangent screw, which sometimes worked a little jerkily. The slit was then accurately adjusted to the focal plane of the sun's image, and the collimator and the eye-telescope were focused carefully for distinct

* The value of one revolution of the micrometer screw (whose head was graduated into 60 divisions,) was about 4′ 9″; but it was continually varying by a slight amount, since in adjusting for distinct vision of the spectrum no pains was taken to keep the distance between the object glass and the cross hairs strictly constant. This will account for the small variations of the measured intervals between the same lines as determined on different occasions—variations considerably exceeding the probable errors of reading.

vision; after this no adjustment of either telescope or spectroscope was touched in the slightest until the observation was complete. The slit being placed nearly tangent to the limb of the sun, and the driving clock started, a series of micrometer readings was made upon the different lines in the group to be observed, first running the micrometer wires one way and then back, thus obtaining two readings for each line. Then the Right Ascension tangent screw was gently turned until the opposite limb was brought to the slit, and the micrometer readings were repeated, running down and back *twice*, so as to give four readings of each line; finally, moving the tangent screw so as to bring back the limb first observed, another set of readings was taken, two on each line, which finished the observation. The object of this arrangement of readings is of course to detect any possible disturbance of the instrument during the work, and to eliminate the effect of the earth's rotation, or of any uniformly progressive change in the relative positions of the collimator, grating, and eye-telescope, due to slight alterations of flexure caused by the motion of the telescope. Each reading given in the following tables is therefore the mean of four. The probable error of a single reading (due to inaccuracy of bisection or instrumental disturbance, but not of course including possible constant errors) was found to be about $\frac{3}{10}$ of one micrometer division; so that the probable error of each reading given in the tables is about 0·15 of a division.

It did not occur to me until near the end of the observations that, with the slit *tangent* to the sun's image, the heat would tend to displace the line of collimation by expanding the slit-plate more on one side than the other, and slightly bending the tube to which it is attached. Since, however, the effect would be in opposite directions according as the grating was inclined so as to throw the reflected slit-image to the right or the left, this effect must be nearly eliminated from the final mean. In some of the observations of Aug. 12, it was eliminated by inverting the spectroscope, i. e., rotating the whole spectroscope 180°, around the line of collimation; this, however, in one of the series of readings always brings the eye-piece into an inconvenient position. One set of observations on Aug. 12, was made with the slit *radial* to the sun's image—in this case the heat of the image has no injurious effect, but it is much more difficult to point the micrometer on the *end* of a line than on its middle, and the probable errors of reading are more than doubled.

The formulæ employed in reducing the observations are as follows: Let U = the relative velocity of two opposite points of the sun's equator, and $d\lambda$ the corresponding change of wave length of a spectrum-line, whose wave length is λ; also let the

velocity of light = 186600 English miles per second, according to the latest determination of Cornu. Then, by Doppler's theory,

$$U = 186600 \times \frac{d\lambda}{\lambda}. \qquad (1)$$

If now, in any group or spectrum lines of small extent, I is put for the difference of wave length of the extreme lines of the group; Δ for the interval between the extreme lines measured in micrometer units, and δ for the displacement as indicated by the difference between the micrometer readings on a given line when the slit is placed on the eastern and western limbs of the sun's image respectively, we shall have

$$d\lambda = I \times \frac{\delta}{\Delta}, \text{ and } U = 186600 \frac{I}{\lambda} \times \frac{\delta}{\Delta}, \qquad (2)$$

where λ of course is to be taken as the *mean* wave length of the group.

Taking Angström's wave lengths, we find for the D group $U = 190{\cdot}3 \frac{\delta}{\Delta}$, and for the 1474 group $U = 361{\cdot}1 \frac{\delta}{\Delta}$; which amounts to saying that a velocity of 190·3 English miles would displace one of the D lines by a space equal to the distance between them, and for the other group a velocity of 361·1 miles would be required.

From the sun's known dimensions and period of rotation, adopting Faye's numbers, the equatorial velocity of its surface is easily found to be 1 248 English miles per second; U, of course, ought to come out double this, or 2·496.

The tables need little explanation; the expression "grating right," means that the grating was so inclined as to throw the reflected image of the slit to that side of the collimator which was remote from the eye-telescope. In this position the spectra were more dispersed, but less satisfactorily defined than those of the same order obtained by turning the grating "left," i. e., toward the eye-telescope. The first column contains the designation of the line observed; the second, headed "west," gives the reading of the micrometer obtained at this limb; the fourth, headed "east," gives the reading of the eastern limb; the third, headed "mean," contains the mean of the numbers in the second and fourth columns, expressed in divisions of the micrometer head; the difference between these, given at the bottom of the column is Δ. The fifth column contains the differences between the numbers in the second and fourth columns, and their mean, given at the bottom of the column, is δ.

In the earlier observations of the D group several other lines were observed besides the three given, but they were so faint that the readings were very discrepant, sometimes to the extent

of two or three divisions, and they are therefore omitted. The readings of the nickel line are retained, though far less reliable than those of the two Ds.

(1.) July 10, 1876; 9.30 to 10.15 A.M. (civil time); grating *right;* spectrum of 6th order; definition poor.

	West.	Mean.	East.		
	r d	d	r d	d	
D_1	4 45·65	284·70	4 43·75	1·90	$\frac{\delta}{\Delta}=0{\cdot}0186$
Ni	3 52·90		3 49·75	2·15	U=3·55
D_2	2 59·00	178·05	2 57·10	1·90	
		$\Delta=106{\cdot}65$		$\delta=1{\cdot}98$	

If we reject the nickel line $\delta'=1{\cdot}90$, $\frac{\delta'}{\Delta}=0{\cdot}0178$ and $U'=3{\cdot}33$.

(2.) July 10; 10.30 to 11.10 A.M. D lines; grating *right;* spectrum of 6th order; definition medium.

	West.	Mean.	East.		
	r d	d	r d	d	
D_1	4 26·40	265·57	4 24·75	1·65	$\frac{\delta}{\Delta}=0{\cdot}0187$
Ni	3 32·05		3 29·65	2·40	
D_2	3 39·95	158·97	3 38·00	1·95	U=3·57
		d			
		$\Delta=106{\cdot}60$		$\delta=2{\cdot}00$	

Rejecting the nickel line, $\delta'=1{\cdot}80$; $\frac{\delta'}{\Delta}=0{\cdot}0169$. $U'=3{\cdot}22$.

(3.) July 10: 11.35 to 12.00 A.M. D lines; grating *left;* spectrum 6th order; definition fine.

	West.	Mean.	East.		
	r	d	r d	d	
D_1	4 13·50	253·87	4 14·25	0·75	$\frac{\delta}{\Delta}=0{\cdot}0126$
Ni	4 51·00		4 51·82	0·82	
D_2	5 16·87	317·28	5 17·70	0·83	U=2·40
		$\Delta=63{\cdot}41$		$\delta=0{\cdot}80$	

(4.) July 15; 10.15 to 10.40 A.M. Observation interrupted by clouds when half completed. D lines; grating *right;* spectrum 6th order; definition poor.

	West.	Mean.	East.		
	r d	d	r d	d	
D_1	5 08·40	308·15	5 07·90	0·50	$\frac{\delta}{\Delta}=0{\cdot}0061$
Ni	4 12·70		4 12·00	0·70	U=1·16
D_2	3 22·05	201·67	3 21·30	0·75	This observation is entitled to little weight.
		$\Delta=106{\cdot}48$		$\delta=0{\cdot}65$	

(5.) August 10; 9.30 to 10.40 A.M. Twice the usual number of readings taken. D lines; grating *left;* spectrum 7th order; definition excellent.

	West. r	d	Mean. d	East. r	d	d	
D_1	0	33·30	33·27	0	34·25	0·95	$\frac{\delta}{\Delta}=0{\cdot}0146$
Ni	1	13·47		1	15·00	1·53	U=2·78
?	1	36·25		1	37·40	1·15	This observation deserves *double* weight, on account of the number of readings and lines observed.
D_2	1	52·92	113·45	1	53·97	1·05	
			d				
			$\Delta=80{\cdot}18$			$\delta=1{\cdot}17$	

(6.) August 10; 11.10 to 11.50 A. M. D lines; grating *right;* spectrum 6th order; definition poor.

	West. r	d	Mean. d	East. r	d	d	
D_2	2	18·70	137·63	2	16·55	2·15	$\frac{\delta}{\Delta}=0{\cdot}0188$
Ni	1	25·65		1	22·85	2·80	U=3·59
D_2	0	32·05	31·52	0	31·00	1·05	
			$\Delta=106{\cdot}11$			$\delta=2{\cdot}00$	

Rejecting the nickel line, $\delta'=1{\cdot}60$, $\frac{\delta'}{\Delta}=0{\cdot}0151$ $U'=2{\cdot}87$.

(7.) August 12; 9.30 to 10.10 A. M. 1474 group; grating *left;* 8th order; definition fine;* instrument *inverted.*

	West. r	d	Mean. d	East. r	d		
$K1463_2$	2	51·72	172·22	2	52·72	1·00	$\frac{\delta}{\Delta}=0{\cdot}00783$
1467	3	51 00		3	52·28	1·28	U=2·66
	4	49·65		4	51·60	1·95	
1474	5	49·20	349·71	5	50·23	1·03	
			$\Delta=177{\cdot}49$			$\delta=1{\cdot}31$	

(8.) August 12; 10.35 to 11.10 A. M. 1474 group; grating *left;* spectrum 8th order; instrument *erect;* definition fine.

	West. r	d	Mean. d	East. r	d	d	
K1474	2	29·90	149·32	2	28·75	1·15	$\frac{\delta}{\Delta}=0{\cdot}00666$
1467	4	28·00		4	26·82	1·18	
1463_2	5	27·63	326·61	5	26·42	1·21	U=2·41
			d			d	
			$\Delta=177{\cdot}29$			$\delta=1{\cdot}18$	

(9.) August 12; 11.30 A. M. to 12.20 P. M. 1474 group; grating *left;* spectrum 8th order; slit *radial;* definition fine.

	West. r	d	Mean. d	East. r	d	d	
$K1463_2$	0	52·92	53·46	0	54·00	1·08	$\frac{\delta}{\Delta}=0{\cdot}00651$
1467	1	52·85		1	54·15	1·30	U=2·35
1474	3	51·18	231·72	3	52·27	1·09	
			$\Delta=178{\cdot}26$			$\delta=1{\cdot}16$	

* The definition was such that 1474 constantly showed double, and on moving the slit to the base of the chromosphere, the bright line which appeared was clearly seen to coincide with the more refrangible of the two components.

Since the points observed were not situated upon the solar equator it is necessary to correct each result by multiplying it by a factor depending upon the heliographic latitude, φ, of the point. If the sun's surface rotated as a coherent mass the factor would be simply, sec. φ. Since this is not the case however, the expression becomes more complicated. Adopting Faye's constants and formula of solar rotation, we find the factor,

$$f=\frac{1}{\cos\varphi\,(1-0{\cdot}216\sin^2\varphi)}.$$

On July 10, $\varphi=2°$, $f=1{\cdot}001$; on July 15, $\varphi=3°$, $f=1{\cdot}002$; on Aug. 10, $\varphi=14°$, $f=1{\cdot}044$; on Aug. 12, $\varphi=15°$, $f=1{\cdot}051$.

Applying the corrections we have the following, in which the column headed Uε, gives the results without discrimination, while the column U′ε contains the results obtained by throwing out the nickel line in observations (1), (2) and (6), and rejecting entirely (4), while (5) is counted twice, as having double weight for the reasons assigned.

	Uε		U′ε
(1)	3·55	(1′)	3·33
(2)	3·57	(2)	3·22
(3)	2·40	(3)	2·40
(4)	1·16	(4)	2·90
(5)	2·90	(5)	2·90
(6)	3·75	(6)	2·99
(7)	2·80	(7)	2·80
(8)	2·53	(8)	2·53
(9)	2·47	(9)	2·47
Mean	2·79 ± 0·18		2·84 ± 0·07

The two results do not differ materially, but the second is much more reliable. It makes the velocity of the sun's rotation 1·42 miles per second, while direct observation gives 1·25; perhaps the difference is no more than might be expected; still the difference of 0·34 between the values of U as determined spectroscopically and directly is so many times larger than the probable error of the spectroscopic result, that I am much inclined, especially considering the agreement with Vogel's result, to think it indicates a physical fact, and that the solar atmosphere really sweeps forward over the underlying surface, in the same way that the equatorial regions outstrip the other parts of the sun's surface. If the equatorial acceleration is produced by external causes such an effect would be likely.

It may be interesting to add, that on Aug. 10, a careful series of readings was made on a principal line of the B group, in the spectrum of the 6th order, with entirely negative results; as was to be expected, since the line is atmospheric. The mean of ten readings at the west limb was $56^d{\cdot}22$, at the east limb $56^d{\cdot}23$.

Hanover, N. H., Sept. 12, 1876.

Art. XXXIX.—*Researches in Acoustics;* by Alfred M. Mayer. Paper No. 8, containing:

1. On the obliteration of the sensation of one sound by the simultaneous action on the ear of another more intense and lower sound.
2. On the discovery of the fact that a sound even when intense cannot obliterate the sensation of another sound lower than it in pitch.
3. On a proposed change in the usual method of conducting orchestral music, indicated by the above discoveries.
4. Applications of the interferences of sonorous sensations to determinations of the relative intensities of sounds.

This communication is preliminary to an elaborate paper on the above subjects. For conciseness and clearness, I present the few facts I have now to offer in the form of notes of experiments:—

1. ***On the obliteration of the sensation of one sound by the simultaneous action on the ear of another more intense and lower sound.***

Experimental Observations on the Obliteration of one Sound by another.—Several feet from the ear I placed one of those loud-ticking spring-balance American clocks, which make four beats in a second. Then I brought quite close to my ear a watch (made by Lange, of Dresden) ticking five times in the second. In this position I heard all the ticks of the watch, even those which coincided with every fourth tick of the clock. Let us call the fifth tick of the watch which coincided with one of the ticks of the clock, its fifth tick. I now gradually removed the watch from the ear, and perceived that the fifth tick became fainter and fainter, till at a certain distance it entirely vanished, and was, so to speak, "stamped out" of the watch.*

Similar and more striking experiments were made with an old silver watch, beating four times to the second, by causing this watch to gain about thirty seconds an hour on the clock, so that at every two minutes the ticks of the watch and clock exactly coincided. When the watch was held near the ear, every one of its ticks was heard distinctly; but on gradually removing it from the ear, the ticks of the watch became fainter

* In the publication of this paper in Nature, Aug. 10, 1876, my friend Mr. Alexander J. Ellis, F.R.S., appends the following note to the above experiment:—"The precise number of ticks in a second here mentioned are not necessary for roughly observing and understanding these phenomena. I observed them by a common American pendulum clock placed on a table, which increased the power of its half-second ticks, and a watch beating five times in two seconds. Rev. Mr. Haweis informs me that he has often noticed a similar effect at night with ordinary watches. The sensation produced by the obliteration of the tick, when the proper distance of the watch from the ear has been attained, and the consequent sudden division of the ticks into periods separated by silences, is very peculiar. It is difficult not to believe that some accident has not suddenly interfered with the action of the watch, instead of merely with our own sensations."—A. J. E.

and fainter at the coincidences, and when the watch had been removed to a distance of nine inches from the ear, the ticks of the watch were utterly obliterated during *three* whole seconds of its ticks about the time of coincidence. On removing the watch to a distance of twenty-four inches, I found that I lost its ticks during *nine* seconds about the time of coincidence. It is here important to remark that the ticks of the clock are *longer* in duration, as well as *lower* in pitch, than those of the watch's. With the watch remaining at the distance of twenty-four inches from the ear, I listened with all my attention, as tick by tick the watch approached the time of coincidence. Since the ticks of the watch are shorter in duration than those of the clock, they are *overlapped* by the other about the time of coincidence. Hence as, so to speak, the short ticks of the watch glided tick after tick, under the long ticks of the clock, I perceived that more and more of the duration of each successive watch-tick became extinguished by the tick of the clock, until only the *tail* end of the short tick of the watch was left audible and at last even this also crept under the long tick of the clock and the whole of the ticks of the watch were rendered inaudible for *nine* seconds, at the end of which time the front or *head* of the watch-tick, as we may call it, protruded beyond the clock-tick, and then slowly grew up into a complete watch-tick as before. In this succession of events the tick of the old silver watch (made by Tobias) disappears with a sharp *chirp*, like a cricket's, and re-appears with a sound like that made by a boy's marble falling upon others in his pocket. By this experiment, therefore, a gradual analysis is made of the effect of the tick of the clock on the tick of the watch, affording a beautiful illustration of the fact that one sonorous sensation may overcome and obliterate another.

Experiments to determine the relative intensity of the Clock-ticks which obliterate the Watch-ticks.—The clock was placed on a post in the middle of an open level field in the country, on nights when the air was calm and noiseless. The ticks of the clock became just inaudible when my ear was removed to a distance of 350 feet. The ticks of the watch became just inaudible at a distance of twenty feet. The ratio of the squares of these numbers makes the ticks of the clock about 300 times more intense than those of the watch. On the same nights that I made the above determinations I also put the clock on the post and placing against my zygomatic process a slender stick graduated to inches and tenths, I stood with my ear at distances from the clock of from eight to sixteen feet, and then slid the watch above and along the stick (taking care that it did not touch it) until it reached such a distance from the ear that its fifth tick just disappeared. Knowing the relative intensities of the ticks of clock and watch when placed at the same distance

from the ear, the law of the reciprocals of the squares gives the relative intensities when the clock and watch are at the several distances obtained in the above experiments. Large numbers of such experiments have been made, and the results agree perfectly well when we take into consideration first, the difficulty thrown in the path of the determinations by the *gradual* fading away of the watch-ticks as they approach coincidence with the clock-ticks; and, secondly, the impossibility of arriving at *any* result at all, if the slightest noise (the rustle of a gentle breeze, the piping of frogs, the bark of a distant dog) should fall on the ear of the observer when engaged in making an experiment. The general result of the numerous experiments thus made shows that the sensation of the watch-tick is obliterated by a coincident tick of the clock, when the intensity of the clock-tick is *three times* that of the watch-tick. This result, however, must be regarded as merely approximative, not only from the manner in which it was obtained, but from the *complexity* of the sounds on which the experiments were made. It is interesting, however, both as being, I believe, the first determination of this kind that has ever been made, and as having opened out a new and important field of research in physiological acoustics.

Experiments on the Interference of the sensations of Musical Sounds.—Reserving the further development of my discoveries for future papers, I will now briefly describe some of the more prominent and simple phenomena, which I discovered in experimenting with *musical sounds.* At the outset I will remove an objection always made by those versed in acoustics, but unacquainted with these new phenomena. It is as follows:—"You say that one sound may obliterate the sensation of another; but are you sure that the real fact is not an alteration of the *quality* of the more intense sound by the action of the concurrent feebler vibration?" I exclude this objection by experimenting as follows:—An open or closed organ-pipe is sounded forcibly, and at a few feet from it is placed the instrument emitting the sound to be obliterated, which may be either a tuning-fork on its resonance box, or a closed organ-pipe communicating with a separate bellows. Suppose that in the following experiment both tuning-fork and closed organ-pipe produce a note higher in pitch than the more intense or extinguishing sound of the open organ-pipe. Now sound the fork alone strongly, and alternately shut and open its resonance box with the hand. We can thus obtain the sound of the fork in a *regular measure of time.* When the ear has well apprehended the intervals of silence and of sound thus produced, begin the experiment by sounding the open pipe and tuning-fork simultaneously Now, if any change is thus effected in the quality of sound emitted by the open pipe, this change cannot occur except when the fork is sounded, and hence, if it occurs at all,

it must occur in the regular measure in which the fork is sounded. The following are the facts really observed. At first every time that the mouth of the box is open, the sound of the fork is distinctly heard, and changes the quality of the note of the open pipe. But as the vibrations of the fork run down in amplitude, the sensations of its effect become less and less, till they soon entirely vanish, and not the slightest change can be observed in the quality or intensity of the note of the open organ-pipe, whether the resonance box of the fork be open or closed. Indeed at this stage of the experiment the vibrations of the fork may be suddenly and totally stopped without the ear being able to detect the fact. But if instead of stopping the fork when it becomes inaudible, we stop the sound of the open organ-pipe, it is impossible not to feel surprised at the strong sound of the fork which the open pipe had smothered and had rendered powerless to affect the ear. If we replace the tuning-fork by a closed organ-pipe of the same pitch, the results will be the same, but in this case I adjust the intensity of the higher closed pipe to the point of extinction by regulating the flow of air from the bellows by a valve worked with a screw. The alternation of sound and silence is obtained by closing and opening the mouth of the closed pipe by the hand.

2. *On the discovery of the fact that a sound even when intense cannot obliterate the sensation of another sound lower than it in pitch.*

High Sounds cannot obliterate Low Sounds.—A new and remarkable fact was now discovered. No sound, even when very intense, can diminish or obliterate the sensation of a concurrent sound which is lower in pitch. This was proved by experiments similar to the last, but differing in having the more intense sound higher (instead of lower) in pitch. In this case, when the ear decides that the sound of the (lower and feebler) tuning-fork is just extinguished, it is generally discovered on stopping the higher sound, that *the fork*, which should produce the lower sound, *has ceased to vibrate*. This surprising experiment must be made in order to be appreciated. I will only remark that very many similar experiments, ranging through four octaves, have been made, with consonant and dissonant intervals, and that scores of different hearers have confirmed this discovery. It is important to understand that this phenomenon depends solely on *difference* of pitch, and not at all on the absolute pitch of the notes. Thus a feeble c''' (1024 double vibrations) is heard as distinctly through an intense e''' (1280 double vibrations) as a feeble c (128 double vibrations) is heard through an intense g (192 double vibrations) or an intense c' (256 double vibrations).

The development of the applications and of the further illustrations of these discoveries would occupy too much space; I must therefore restrict myself to mentioning some of the most interesting. Let a man read a sentence over and over again with the same tone and modulation of voice, and while he is so doing forcibly sound a c' pipe (256 double vibrations). A remarkable effect is produced, which varies somewhat with the voice experimented on, but the ordinary result is as follows. It appears as though two persons were reading together, one with a grave voice (which is found by the combination of all the reader's real vocal sounds below c in pitch, or having less than 256 double vibrations), the other with a high-pitched voice, generally squeaky and nasal, and, I need not add, very disagreeable. Of course the aspirates come out with a distressing prominence. I have observed many curious illustrations of this change in the quality of the tone of the voice, caused by the entire or partial obliteration of certain vocal components, while listening to persons talking during the sound of a steam whistle, or in one of our long, resonant American railway carriages. Experiments similar to those on the human voice, can be made, with endless modifications, on other composite sounds, as those of reed-pipes, of stringed instruments, of running water, &c. With one of my c (128 double vibrations) free Grenié reeds, I get very marked results. Using as a concurrent sound an intense c' (256 double vibrations) I perceive the prime or fundamental simple tone c to be unaffected in intensity, while all the other partial tones (higher harmonics or overtones, as they are sometimes called) are almost obliterated, except the fifth partial (or fourth upper partial e'', of 640 double vibrations, and the sixth partial (or fifth upper partial) g'' (of 768 double vibrations), which come out with wonderful distinctness. The fact that the lowest, or prime partial tone in the majority of ordinary compound musical tones is strongest, is due (among other reasons) to the fact that the sensation of each partial tone of which the whole musical tone is composed, is diminished by the action on the ear of all the components or partial tones, *below* it in pitch. Thus the higher the pitch of any component or partial tone, the greater the number of lower components which tend to obliterate it. But the prime, or lowest component partial tone, is not affected by any other. Another illustration I cannot resist giving. At the end of the street in New York, in which I resided, there is a large fire-alarm bell, the residual sound of which, after its higher components have disappeared, is a deep simple tone. This bass sound holds its own with total indifference to the clatter of horses, or to any sounds *above* it in pitch. It dies out with a smooth gradient, generally without the slightest indentation or break produced by the other sounds of the street. Indeed, in this case, as in all others where one

sound remains unaffected by intense higher notes, the observer feels as though he had a special sense for the perception of the graver sound—an organ entirely distinct from that which receives the impress of the higher tones.

That one sonorous sensation cannot interfere with another which is lower in pitch, is a remarkable physiological discovery, and next after the demonstration of the fact that the ear is capable of analyzing compound musical sounds into their constituent or partial simple tones, is probably the most important addition yet made to our knowledge of the nature of hearing. It cannot fail to introduce profund modifications into the hypotheses heretofore framed respecting the mechanism and functions of the ear.

3. *On a proposed change in the usual method of conducting Orchestral Music, indicated by the above discoveries.*

We have seen how an intense sound may obliterate, entirely or in part, the sensations of certain partial tones or components of any musical tone, and thus produce a profound change in its quality. In a large orchestra I have repeatedly witnessed the entire obliteration of all sounds from violins, by the deeper and more intense sounds of the wind instruments, the double-bases alone holding their own. I have also observed the sounds of the clarinets lose their peculiar quality of tone and consequent charm from the same cause. No doubt the conductor of the orchestra heard all his violins, ranged as they always are close around him, and did not perceive that his clarinets had lost that quality of tone on which *the composer* had relied for producing a special character of expression.

The function of the conductor of an orchestra seems to be threefold. First, to regulate and fix the time. Secondly, to regulate the intensity of the sounds produced by the individual instruments, for the purpose of expression. Thirdly, to give the proper quality of tone or *feeling* to the whole sound of his orchestra, considered as a single instrument, by regulating *the relative intensities* of the sounds produced by the various classes of instruments employed. Now this third function, the regulation of relative intensities, has hitherto been discharged through the judgment of the ears of a conductor who is placed in the most disadvantageous position for judging by his ears. Surely he is not conducting for his own personal gratification, but for the gratification of his audience, whose ears stand in very different relations from his own in respect to their distance from the various instruments in action. Is it not time that he should pay more attention to his third function and place himself in the position occupied by an average hearer? This position would be elevated, and somewhere in the midst of the audience. The exact determination of its place would depend on various

conditions which cannot now be considered. That the position at present occupied by the conductor of an orchestra has often allowed him to deprive his audience of some of the most delicate and touching qualities of orchestral and concerted vocal music I have no doubt, and I firmly believe that when he changes his position in the manner now proposed the audience will have some of that enjoyment which he has too long kept to himself. During the past winter, in the Academy of Music at New York, and this spring at Offenbach's concerts, I fully confirmed all the foregoing surmises, by placing myself in different parts of the house to observe the different results, and my opinions were fully shared by others who have a more delicate musical organization than I can lay claim to.

In large orchestras, these interferences of sonorous sensations are so multiplied and various as to be beyond our mental conception. By taking them up in detail, some general laws may, however, be evolved. But it will be impossible to formulate such laws until, firstly, we are in possession of a *quantitative* analysis of the compound tones of all musical instruments (that is, until we know the relative loudness of the partial tones of which they are composed at all parts of their compass), and secondly, we have determined throughout the musical scale the relative intensities of the sounds (of simple tones) when obliteration of the sensations of higher (simple) tones supervenes. The powerlessness of one sound to affect the sensation due to another sound lower than itself in pitch greatly simplifies this problem.

4. *Applications of the interferences of sonorous sensations to the determinations of the Relative Intensities of Sounds.*

Quantitative analysis of the compound tones of musical instruments is now the great desideratum of the composer. It is only after we know the relative intensities of the components of typical musical tones used in orchestral performances, that we can so regulate their intensities as to give those qualities of sound which the composer desires to be heard. Thus, it at once becomes evident that the instruments used in orchestral music should be very differently constructed from those used for solos or quartets. In orchestral instruments certain *characteristic* upper partials (overtones, harmonics) should predominate, in order to find expression in the midst of other and graver sounds. Such orchestral instruments will therefore have exaggerated peculiarities in their qualities of tone, which will render them unfit to be played on alone, and uninfluenced by other orchestral notes. It is surely not hopeless to anticipate that empirical rules may be attained, which will guide the musical instrument-maker to the production of those special qualities of tone required in orchestral instruments. It is fortu-

nate that the very phenomena of the interferences of sonorous sensations will assist in the much desired solution of the problem of measuring the intensity of a sound (simple tone), either when existing alone or as component of an ordinary musical (compound) tone. On this subject I am now engaged. It is evident (by way of illustration), that so far as concerns the measure of the relative intensities of sounds *of the same pitch*, this problem has already received the simplest solution by merely placing these sounds at various distances, and obliterating the sensations they excite by means of a constant and standard sound of a lower pitch. But I reserve a description of this work for a more formal publication.

ART. XL.—*Address at the Glasgow Meeting of the British Association;* by Professor SIR WILLIAM THOMSON, President of the Mathematical and Physical Section.

A CONVERSATION which I had with Professor Newcomb one evening last June, in Professor Henry's drawing-room in the Smithsonian Institution, Washington, has forced me to give all my spare thoughts ever since to Hopkins's problem of Precession and Nutation, assuming the earth a rigid spheroidal shell filled with liquid. Six weeks ago, when I landed in England after a most interesting trip to America and back, and became painfully conscious that I must have the honor to address you here to-day, I wished to write an address of which science in America should be the subject. I came home, indeed, vividly impressed with much that I had seen, both in the Great Exhibition of Philadelphia and out of it, showing the truest scientific spirit and devotion, the originality, the inventiveness, the patient persevering thoroughness of work, the appreciativeness, and the generous openmindedness and sympathy, from which the great things of science come.

Θέλω λέγειν Ἀτρειδας
Θέλω δὲ Κυδμὸν ᾄδειν.

I wish I could speak to you of the veteran Henry, generous rival of Faraday in electromagnetic discovery; of Peirce the founder of high mathematics in America; of Bache, and of the splendid heritage he has left to America and to the world in the United States Coast Survey; of the great school of astronomers which followed, Gould, Newton, Newcomb, Watson, Young, Alvan Clarke, Rutherfurd, Draper, father and son: of Commander Belknap and his great exploration of the Pacific depths by pianoforte wire, with imperfect apparatus supplied from Glasgow, out of which he forced a success in his own way; of Cap-

tain Sigsbee, who followed with like fervor and resolution, and made further improvements in the apparatus by which he has done marvels of easy, quick, and sure deep-sea sounding in his little surveying ship *Blake*; and of the admirable official spirit which makes such men and such doings possible in the United States Naval Service. I would like to tell you too of my reason for confidently expecting that American hydrography will soon supply the data from tidal observations, long ago asked of our Government in vain by a Committee of the British Association, by which the amount of the earth's elastic yielding to the distorting influence of the sun and moon will be measured; and of my strong hope that the Compass Department of the American Navy will repay the debt to France, England, and Germany so appreciatively acknowledged in their reprint of the works of Poisson, Airy, Archibald Smith, Evans, and the Liverpool Compass Committee, by giving in return a fresh marine survey of terrestrial magnetism, to supply the navigator with data for correcting his compass without sights of sun or stars.

Can I go on to precession and nutation without a word of what I saw in the Great Exhibition of Philadelphia? In the U. S. Government part of it, Professor Hilgard showed me the measuring-rods of the U. S. Coast Survey, with their beautiful mechanical appliances for end measurement, by which the three great base lines of Maine, Long Island, and Georgia, were measured with about the same accuracy as the most accurate scientific measurers, whether of Europe or America, have attained in comparing two meter or yard measures.

In the United States telegraphic department I saw and heard Elisha Gray's splendidly worked-out electric telephone actually sounding four messages simultaneously on the Morse code, and clearly capable of doing yet four times as many with very moderate improvements of detail; and I saw Edison's automatic telegraph delivering 1,015 words in 57 seconds: this done by the long-neglected electro-chemical method of Bain, long ago condemned in England to the helot work of recording from a relay, and then turned adrift as needlessly delicate for that. In the Canadian department I heard "To be or not to be, ... there's the rub," through an electric telegraph wire; but, scorning monosyllables, the electric articulation rose to higher flights, and gave me passages taken at random from the New York newspapers:—"S. S. Cox has arrived" (I failed to make out the S. S. Cox); "The City of New York," "Senator Morton," "The Senate has resolved to print a thousand extra copies;" "The Americans in London have resolved to celebrate the coming Fourth of July." All this my own ears heard, spoken to me with unmistakable distinctness by the thin circular disc armature of just such another little electro-magnet as this which

I hold in my hand. The words were shouted with a clear and loud voice by my colleague-judge, Professor Watson, at the far end of the telegraph wire, holding his mouth close to a stretched membrane, such as you see before you here, carrying a little piece of soft iron, which was thus made to perform in the neighborhood of an electro-magnet in circuit with the line motions proportional to the sonorific motions of the air. This, the greatest by far of all the marvels of the electric telegraph, is due to a young countryman of our own, Mr. Graham Bell, of Edinburgh and Montreal, and Boston, now becoming a naturalized citizen of the United States. Who can but admire the hardihood of invention which devised such very slight means to realize the mathematical conception that, if electricity is to convey all the delicacies of quality which distinguish articulate speech, the strength of its current must vary continuously and as nearly as may be in simple proportion to the velocity of a particle of air engaged in constituting the sound?

The Patent Museum of Washington, an institution of which the nation is justly proud, and the beneficent working of the United States patent laws, deserve notice in the section of the British Association concerned with branches of science to which nine-tenths of all the useful patents of the world owe their foundations. I was much struck with the prevalence of patented inventions in the Exhibition: it seemed to me that every good thing deserving a patent was patented. I asked one inventor of a very good invention "Why don't you patent it in England?" He answered, "The conditions in England are too onerous." We certainly are far behind America's wisdom in this respect. If Europe does not amend its patent laws (England in the opposite direction to that proposed in the Bills before the last two sessions of Parliament) America will speedily become the nursery of useful inventions for the world.

I should tell you also of "Old Prob's" weather warnings, which cost the nation 250,000 dollars a year; money well spent say the western farmers, and not they alone: in this the whole people of the United States are agreed, and though Democrats or Republicans playing the "economical ticket" may for half a session stop the appropriations for even the United States Coast Survey, no one would for a moment think of proposing to starve "Old Prob;" and now that 80 per cent of his probabilities have proved true, and General Myers has for a month back ceased to call his daily forecasts "probabilities" and has begun to call them indications, what will the western farmers call him this time next year?

And the United States Naval Observatory, full of the very highest science, under the command of Admiral Davis! If, to get on to precession and nutation, I had resolved to omit

telling you that I had there, in an instrument for measuring photographs of the transit of Venus—shown me by Professor Harkness, a young Scotsman attracted into the United States Naval Service—seen for the first time in an astronomical observatory a geometrical slide, the verdict on the disaster on board the *Thunderer*, published while I am writing this address, forbids me to keep any such resolution, and compels me to put the question, Is there in the British Navy, or in a British steamer, or in a British land boiler another safety-valve so constructed that by any possibility, at any temperature, or under any stress it can jam? and to say that if there is it must be instantly corrected or removed.

I ought to speak to you, too, of the already venerable Harvard University, the Cambridge of America, and of the Technological Institute of Boston, created by William Rogers, brother of my late colleague in this university (Glasgow), Henry Rogers, and of the Johns Hopkins University of Baltimore, which with its youthful vigor has torn Sylvester from us, has utilised the genius and working power of Rowland for experimental research, and three days after my arrival in America, sent for the young Porter Poinier to make him a Fellow. But he was on his deathbed in New York "begging his physicians to keep him alive just to finish his book, and then he would be willing to go." Of his book, "Thermodynamics," we may hope to see at least a part, for much of the manuscript, and good and able friends to edit it, are left; but the appointment to a Fellowship in the Johns Hopkins University came a day too late to gratify his noble ambition.

But the stimulus of intercourse with American scientific men left no place in my mind for framing, or attempting to frame a report on American science. Disturbed by Newcomb's suspicions of the earth's irregularities as a Time-keeper, I could think of nothing but precession and nutation, and tides and monsoons, and settlements of the equatorial regions, and melting of polar ice. Week after week passed before I could put down two words which I could read to you here to-day: and so I have nothing to offer you for my Address but—

Review of Evidence regarding the Physical Condition of the Earth; its Internal Temperature; the Fluidity or Solidity of its Interior Substance; the Rigidity, Elasticity, Plasticity, of its External Figure; and the Permanence or Variability of its Period and Axis of Rotation.

The evidence of a high internal temperature is too well known to need any quotation of particulars at present. Suffice it to say that below the uppermost ten meters stratum of rock or soil sensibly affected by diurnal and annual variations of tem-

perature, there is generally found a gradual increase of temperature downward, approximating roughly, in ordinary localities, to an average rate of 1° C. per thirty meters of descent, but much greater in the neighborhood of active volcanoes, and certain other special localities of comparatively small area, where hot springs and, perhaps, also, sulphurous vapors prove an intimate relationship to volcanic quality. It is worthy of remark in passing, that, so far as we know at present, there are no localities of exceptionally *small* rate of augmentation of underground temperature, and none where temperature diminishes at any time through any considerable depth downward below the stratum sensibly influenced by summer heat and winter cold. Any considerable area of the earth of, say, not less than a kilometer in any horizontal diameter, which for several thousand years had been covered by snow or ice, and from which the ice had melted away and left an average surface temperature of 13°, would, during nine hundred years, show a decreasing temperature for some depth down from the surface: and thirty-six hundred years after the clearing away of the ice would still show a residual effect of the ancient cold, in a half rate of augmentation of temperature downward in the upper strata, gradually increasing to the whole normal rate which would be sensibly reached at a depth of 600 meters.

By a simple effort of geological calculus it has been estimated that 1° per 30 meters gives 1000° per 30,000 meters, and 3333° per 100 kilometers. This arithmetical result is irrefragable, but what of the physical conclusion drawn from it with marvellous frequency and pertinacity that at depths of from 30 to 100 kilometers the temperatures are so high as to melt all substances composing the earth's upper crust? It has been remarked, indeed, that if observation showed any diminution or augmentation of the rate of increase of undergroud temperature in great depths, it would not be right to reckon on the uniform rate of 1° per 30 meters, or thereabouts, down to 30 or 60 or 100 kilometers. "But observation has shown nothing of the kind, and therefore surely it is most consonant with inductive philosophy to admit no great deviation in any part of the earth's solid crust from the rate of increase proved by observation as far as the greatest depths to which we have reached!" Now I have to remark upon this argument that the greatest depth to which we have reached in observations of underground temperature is scarcely one kilometer; and that, if a 10 per cent diminution of the rate of augmentation of underground temperature downward were found at a depth of one kilometer, this would demonstrate* that within the last 100,000 years the

*** For proof of this and following statements regarding Underground Heat I refer to "Secular Cooling of the Earth," *Transactions* of the Royal Society of Edinburgh, 1862, and Thomson and Tait's "Natural Philosophy," Appendix D.**

upper surface of the earth must have been at a higher temperature than that now found at the depth of one kilometer. Such a result is no doubt to be found by observation in places which have been overflown by lava in the memory of man, or a few thousand years farther back; but if, without going deeper than a kilometer, a 10 per cent diminution of the rate of increase of temperature downward were found for the whole earth, it would limit the whole of geological history to within 100,000 years, or, at all events, would interpose an absolute barrier against the continuous descent of life on the earth from earlier periods than 100,000 years ago. Therefore, although search in particular localities for a diminution of the rate of augmentation of underground temperature in depths of less than a kilometer may be of intense interest, as helping us to fix the dates of extinct volcanic actions which have taken place within 100,000 years or so, we know enough from thoroughly sure geological evidence not to expect to find it, except in particular localities, and to feel quite sure that we shall not find it under any considerable portion of the earth's surface. If we admit as possible any such discontinuity within 900,000 years, we might be prepared to find a sensible diminution of the rate at three kilometers depth; but not at anything less than 30 kilometers if geologists validly claim as much as 90,000,000 of years for the length of the time with which their science is concerned. Now this implies a temperature of 1000° C. at the depth of 30 kilometers, allows something less than 2000° for the temperature at 60 kilometers, and does not require much more than 4000° C. at any depth, however great; but does require at the great depths a temperature of at all events not less than about 4000° C. It would not take much "hurrying up" of the actions with which they are concerned, to satisfy geologists with the more moderate estimate of 50,000,000 of years. This would imply at least about 3000° C. for the limiting temperature at great depths. If the actual substance of the earth, whatever it may be, rocky or metallic, at depths of from 60 to 100 kilometers, under the pressure actually there experienced by it can be solid at temperatures of from 3000° to 4000°, then we may hold the former estimate (90,000,000) to be as probable as the latter (50,000,000) so far as evidence from underground temperature can guide us. If 4000° would melt the earth's substance at a depth of 100 kilometers, we must reject the former estimate, though we might still admit the latter; if 3000° would melt the substance at a depth of 60 kilometers, we should be compelled to conclude that 50,000,000 of years is an over-estimate. Whatever may be its age, we may be quite sure the earth is solid in its interior; not, I admit, throughout its whole volume, for there certainly are spaces in

volcanic regions occupied by liquid lava: but whatever portion of the whole mass is liquid, whether the waters of the ocean or melted matter in the interior, these portions are small in comparison with the whole, and we must utterly reject any geological hypothesis which, whether for explaining underground heat or ancient upheavals and subsidences of the solid crust, or earthquakes, or existing volcanoes, assumes the solid earth to be a shell of 30, or 100, or 500, or 1,000 kilometers thickness, resting on an interior liquid mass.

This conclusion was first arrived at by Hopkins, who may therefore properly be called the discoverer of the earth's solidity. He was led to it by a consideration of the phenomena of precession and nutation, and gave it as shown to be highly probable, if not absolutely demonstrated, by his confessedly imperfect and tentative investigation. But a rigorous application of the perfect hydrodynamical equations leads still more decidedly to the same conclusion.

I am able to say this to you now in consequence of the conversation with Professor Newcomb to which I have already alluded. Admitting fully my evidence for the rigidity of the earth from the tides, he doubted the argument from precession and nutation. Trying to recollect what I had written on it fourteen years ago in a paper on the Rigidity of the Earth, published in the *Transactions* of the Royal Society, my conscience smote me, and I could only stammer out that I had convinced myself that so and so, and so and so, at which I had arrived by a non-mathematical short cut, were true. He hinted that viscosity might suffice to render precession and nutation the same as if the earth were rigid, and so vitiate the argument for rigidity. This I could not for a moment admit any more than when it was first put forward by Delaunay. But doubt entered my mind regarding the so and so, and so and so; and I had not completed the night journey to Philadelphia which hurried me away from our unfinished discussion before I had convinced myself that they were grievously wrong. So now I must request as a favor that each one of you on going home will instantly turn up his or her copies of the *Transactions* of the Royal Society for 1862, and of Thomson and Tait's "Natural Philosophy," vol. i., and draw the pen through §§ 23–31 of my paper on the "Rigidity of the Earth" in the former, and through everything in §§ 847–849 of the latter, which refers to the effect on precession and nutation of an elastic yielding of the earth's surface.

When those passages were written I knew little or nothing of vortex motion; and until my attention was recalled to them by Professor Newcomb, I had never once thought of this subject in the light thrown upon it by the theory of the quasi-rigidity

induced in a liquid by vortex motion which has of late occupied me so much. With this fresh light a little consideration sufficed to show me that (although the old obvious conclusion is of course true, that if the inner boundary of the imagined rigid shell of the earth were rigorously spherical, the interior liquid could experience no precessional or nutational influence from the pressure on its bounding surface, and therefore if homogeneous could have no precession or nutation at all, or if heterogeneous only as much precession and nutation as would be produced by attraction from without in virtue of non-sphericity of its surfaces of equal density, and therefore the shell would have enormously more rapid precession and nutation than it actually has—forty times as much, for instance, if the thickness of the shell is sixty kilometers) a very slight deviation of the inner surface of the shell from perfect sphericity would suffice, in virtue of the quasi-rigidity due to vortex motion, to hold back the shell from taking sensibly more precession than it would give to the liquid, and to cause the liquid (homogeneous or heterogeneous) and the shell to have sensibly the same precessional motion as if the whole constituted one rigid body. But it is only because of the very long period (26,000 years) of precession, in comparison with the period of rotation (one day), that a very slight deviation from sphericity would suffice to cause the whole to move as if it were a rigid body. A little further consideration showed me—

(1) That an ellipticity of inner surface equal to $\frac{1}{26000\times365}$ would be too small, but that an ellipticity of one or two hundred times this amount would not be too small, to compel approximate equality of precession throughout liquid and shell.

(2) That with an ellipticity of interior surface equal to $\frac{1}{3}\frac{1}{0}\frac{1}{0}$, if the precessional motive were 26,000 times as great as it is, the motion of the liquid would be very different from that of a rigid mass rigidly connected with the shell:

(3) That with the actual forces and the supposed interior ellipticity of $\frac{1}{3}\frac{1}{0}\frac{1}{0}$ the lunar nineteen-yearly nutation might be affected to about five per cent of its amount by interior liquidity.

(4) Lastly, that the lunar semi-annual nutation must be largely, and the lunar fortnightly nutation enormously, affected by interior liquidity.

But although so much could be foreseen readily enough, I found it impossible to discover, without thorough mathematical investigation, what might be the characters and amounts of the deviations from a rigid body's motion which the several cases of precession and nutation contemplated would present. The investigation, limited to the case of a homogeneous liquid en-

closed in an ellipsoidal shell, has brought out results which I confess have greatly surprised me. When the interior ellipticity of the shell is just too small, or the periodic speed of the disturbance just too great to allow the motion of the whole to be sensibly that of a rigid body, the deviation first sensible renders the precessional or nutational motion of the shell smaller than if the whole were rigid, instead of greater, as I expected. The amount of this difference bears the same proportion to the actual precession or nutation as the fraction measuring the periodic speed of the disturbance (in terms of the period of rotation as unity) bears to the fraction measuring the interior ellipticity of the shell; and it is remarkable that this result is independent of the thickness of the shell, assumed however to be small in proportion to the earth's radius. Thus in the case of precession the effect of interior liquidity would be to diminish the periodic speed of the precession in the proportion stated; in other words, it would add to the precessional period a number of days equal to the multiple of the rotational period equal to the number whose reciprocal measures the ellipticity. Thus in the actual case of the earth if we still take $\frac{1}{3}\frac{1}{0}\frac{1}{0}$ as the ellipticity of the inner boundary of the supposed rigid shell, the effect would be to augment by 300 days the precessional period of 2,600 years, or to diminish by about $\frac{1}{6}\frac{1}{0}''$ the annual precession of about 51″—an effect which I need not say would be wholly insensible. But on the lunar nutation of 18·6 years period the effect of interior liquidity would be quite sensible; 18·6 years being 23 times 300 days, the effect would be to diminish the axes of the ellipse which the earth's pole describes in this period each by $\frac{1}{2}\frac{1}{3}$ of its own amount. The semi-axes of this ellipse calculated on the theory of perfect rigidity from the very accurately known amount of precession and the fairly accurate knowledge which we have of the ratio of the lunar to the solar part of the precessional motive are 9″·22 and 6″·86, with an uncertainty not amounting to one-half per cent on account of want of perfect accuracy in the latter part of data. If the true values were less each by $\frac{1}{2}\frac{1}{3}$ of its own amount, the discrepance might have escaped detection, or might *not* have escaped detection; but certainly could be found if looked for. So far nothing can be considered as absolutely proved with reference to the interior solidity of the earth from precession and nutation: but now think of the solar semi-annual and the lunar fortnightly nutations. The period of each of these is less than 300 days. Now the hydrodynamical theory shows that irrespective of the thickness of the shell, the nutation of the crust would be zero if the period of the nutational disturbance were 300 times the period of rotation (the ellipticity being $\frac{1}{3}\frac{1}{0}\frac{1}{0}$): if the nutational period were anything between

this and a certain smaller critical value depending on the thickness of the crust, the nutation would be negative; if the period were equal to this second critical value, the nutation would be infinite: and if the period were still less, the nutation would be again positive. Further, the 183 days period of the solar nutation falls so little short of the critical 300 days, that the amount of the nutation is not sensibly influenced by the thickness of the crust—is negative and equal in absolute value to $\frac{6}{3}\frac{1}{9}$ (being the reciprocal of $\frac{3}{1}\frac{9}{6}\frac{6}{5}-1$) times what the amount would be were the earth solid throughout. Now this amount as calculated in the Nautical Almanac makes 0″·55, and 0″·51 the semi-axes of the ellipse traced by the earth's axis round its mean position; and if the true nutation placed the earth's axis on the opposite side of an ellipse having ″·86 and ″·81 for its semi-axes, the discrepance could not possibly have escaped detection. But lastly, think of the lunar fortnightly nutation. Its period is $\frac{1}{2}\frac{1}{6}$ of 300 days, and its amount, calculated in the Nautical Almanac on the theory of complete solidity, is such that the greater semi-axis of the approximately circular ellipse described by the pole is 0″·0325. Were the crust infinitely thin this nutation would be negative, but its amount nineteen times that corresponding to solidity. This would make the greater semi-axis of the approximately circular ellipse described by the pole amount to 19×0″·0885, which is 1″·7. It would be negative and of some amount between 1″·7 and infinity, if the thickness of the crust were anything from zero to 120 kilometers. This conclusion is absolutely decisive against the geological hypothesis of a thin rigid shell full of liquid.

But interesting in a dynamical point of view as Hopkin's problem is, it cannot afford a decisive argument against the earth's interior liquidity. It assumes the crust to be perfectly stiff and unyielding in its figure. This of course it cannot be, because no material is infinitely rigid; but, composed of rock and possibly of continuous metal in the great depths, may the crust not as a whole be still enough to practically fulfill the condition of unyieldingness? No; decidedly it could not; on the contrary, were it of continuous steel and 500 kilometers thick, it would yield very nearly as much as if it were india-rubber, to the deforming influences of centrifugal force and of the sun's and moon's attractions. Now, although the full problem of precession and nutation, and what is now necessarily included in it, tides—in a continuous revolving liquid spheroid, whether homogeneous or heterogeneous, has not yet been coherently worked out, I think I see far enough towards a complete solution to say that precession and nutations will be practically the same in it as in a solid globe, and that the tides will be practically the same as those of the equilibrium

theory. From this it follows that precession and nutations of the solid crust, with the practically perfect flexibility which it would have even though it were 100 kilometers thick and as stiff as steel, would be sensibly the same as if the whole earth from surface to center were solid and perfectly stiff. Hence precession and nutations yield nothing to be said against such hypotheses as that of Darwin,* that the earth as a whole takes approximately the figure due to gravity and centrifugal force, because of the fluidity of the interior and the flexibility of the crust. But alas for this "attractive sensational idea that a molten interior to the globe underlies a thin superficial crust; its surface agitated by tidal waves and flowing freely towards any issue that may here and there be opened for its outward escape," as Poulett Scrope called it! the solid crust would yield so freely to the deforming influence of sun and moon that it would simply carry the waters of the ocean up and down with it, and there would be no sensible tidal rise and fall of water relatively to land.

The state of the case is shortly this:—The hypothesis of a perfectly rigid crust containing liquid violates physics by assuming preternaturally rigid matter and violates dynamical astronomy in the solar semi-annual and lunar fortnightly nutations; but tidal theory has nothing to say against it. On the other hand the tides decide against any crust flexible enough to perform the nutations correctly with a liquid interior, or as flexible as the crust must be unless of preternaturally rigid matter.

But now thrice to slay the slain; suppose the earth this moment to be a thin crust of rock or metal resting on liquid matter. Its equilibrium would be unstable! And what of the upheavals and subsidences? They would be strikingly analogous to those of a ship which has been rammed: one portion of crust up and another down, and then all down. I may say with almost perfect certainty, that, whatever may be the relative densities of rock, solid and melted, at or about the temperature of liquifaction, it is, I think, quite certain that cold solid rock is denser than hot melted rock: and no possible degree of rigidity in the crust could prevent it from breaking in pieces and sinking wholly below the liquid lava. Something like this may have gone on and probably did go on for thousands of years after solidification commenced; surface portions of the melted material losing heat, freezing, sinking immediately, or growing to thicknesses of a few meters when the surface would be cool and the whole solid dense enough to sink. "This

* "Observations on the Parallel Roads of Glen Roy and other Parts of Lochaber in Scotland, with an attempt to prove that they are of Marine Origin."—*Transactions* of the Royal Society for Feb., 1839, p. 81.

process must go on until the sunk portions of crust build up from the bottom a sufficiently close-ribbed skeleton or frame, to allow fresh incrustations to remain bridging across the now small areas of lava, pools or lakes.

"In the honey-combed solid and liquid mass thus formed, there must be a continual tendency for the liquid, in consequence of its less specific gravity, to work its way up; whether by masses of solid falling from the roofs of vesicles or tunnels, and causing earthquake shocks, or by the roof breaking quite through when very thin, so as to cause two such hollows to unite or the liquid of any of them to flow out freely over the outer surface of the earth; or by gradual subsidence of the solid owing to the thermodynamic melting, which portions of it under intense stress must experience according to my brother's theory. The results which must follow from this tendency seem sufficiently great and various to account for all that we learn from geological evidence of earthquakes, of upheavals and subsidences of solid, and of eruptions of melted rock."*

Leaving altogether now the hypothesis of a hollow shell filled with liquid, we must still face the question, how much does the earth, solid throughout, except small cavities or vesicles filled with liquid, yield to the deforming (or tide-generating) influences of sun and moon? This question can only be answered by observation. A single infinitely accurate spirit-level or plummet far enough away from the sea to be not sensibly affected by the attraction of the rising and falling water would enable us to find the answer. Observe by level or plummet the changes of direction of apparent gravity relatively to an object rigidly connected with the earth, and compare these changes with what they would be were the earth perfectly rigid, according to the known masses and distances of sun and moon. The discrepance, if any is found, would show distortion of the earth and would afford data for determining the dimensions of the elliptic spheroid into which a non-rotating globular mass of the same dimensions and elasticity as the earth would be distorted by centrifugal force if set in rotation, or by the tide-generating influence of sun or moon. The effect on the plumb-line of the lunar tide-generating influence is to deflect it towards or from the point of the horizon nearest to the moon, according as the moon is above or below the horizon. The effect is zero when the moon is on the horizon or overhead, and is greatest in either direction when the moon is 45° above or below the horizon. When this greatest value is reached, the plummet is drawn from its mean position through

* "Secular Cooling of the Earth." *Transactions* of the Royal Society of Edinburgh, 1862 (W. Thomson), and Thomson and Tait's "Natural Philosophy," §§ (*e e*), (*f f*).

a space equal to $\frac{1}{12000000}$ of the length of the thread. No ordinary plummet or spirit-level could give any perceptible indication whatever of this effect; and to measure its amount it would be necessary to be able to observe angles as small as $\frac{1}{120000000}$ of the radius, or about $\frac{1}{600}''$. Siemens' beautiful hydrostatical multiplying level may probably supply the means for doing this. Otherwise at present no apparatus exists within small compass by which it could be done. A submerged water-pipe of considerable length, say twelve kilometers, with its two ends turned up and open might answer. Suppose, for example, the tube to lie North and South, and its two ends to open into two small cisterns, one of them, the southern, for example, of half a decimeter diameter (to escape disturbance from capillary attraction): and the other of two or three decimeters diameter (so as to throw nearly the whole rise and fall into the smaller cistern). For simplicity suppose the time of observation to be when the moon's declination is zero. The water in the smaller or southern cistern will rise from its lowest position to its highest position while the moon is rising to maximum altitude, and fall again after the moon crosses the meridian till she sets: and it will rise and fall again through the same range from moonset to moonrise. If the earth were perfectly rigid, and if the locality were in latitude 45°, the rise and fall would be half a millimeter on each side of the mean level; or a little short of half a millimeter if the place is within 10° north or south of latitude 45°. If the air were so absolutely quiescent during the observations as to give no varying differential pressure on the two water surfaces to the amount of $\frac{1}{100}$ millimetre of water, or $\frac{1}{1400}$ of mercury, the observation would be satisfactorily practicable, as it would not be difficult by aid of a microscope to observe the rise and fall of the water in the smaller cistern to $\frac{1}{100}$ of a millimeter; but no such quiescence of the atmosphere could be expected at any time, and it is probable that the variations of the water-level due to difference of the barometric pressure at the two ends would in all ordinary weather quite overpower the small effect of the lunar tide-generating motive. If, however, the two cisterns instead of being open to the atmosphere were connected air-tightly by a return pipe with no water in it, it is probable that the observation might be successfully made: but Siemens' level or some other apparatus on similarly small scale would probably be preferable to any elaborate method of obtaining the result by aid of very long pipes laid in the ground; and I have only called your attention to such an ideal method as leading up to the natural phenomenon of tides.

Tides in an open canal or lake of twelve kilometers length would be of just the amount which we have estimated for the cisterns connected by submerged pipe; but would be enor-

mously more disturbed by wind and variations of atmospheric pressure. A canal or lake of 240 kilometers length, in a proper direction in a suitable locality, would give but ten millimeters rise and fall at each end, an effect which might probably be analyzed out of the much greater disturbance produced by wind and differences of barometric pressure; but no open liquid level short of the *ingens æquor*, the ocean, will probably be found so well adapted as it for measuring the absolute value of the disturbance produced on terrestrial gravity by the lunar and solar tide-generating motive. But observations of the diurnal and semi-diurnal tides in the ocean, do not (as they would on smaller and quicker levels) suffice for this purpose, because their amounts differ enormously from the equilibrium values on account of the smallness of their periods in comparison with the periods of any of the grave enough modes of free vibration of the ocean as a whole. On the other hand the lunar fortnightly declinational and the lunar monthly elliptic and the solar semi-annual and annual elliptic tides have their periods so long that their amounts must certainly be very approximately equal to the equilibrium values.

But there are large annual and semi-annual changes of sea-level, probably both differential on account of wind and differences of barometric pressure and differences of temperature of the water, and absolute depending on rain-fall and the melting away of snow and return evaporation, which altogether swamp the small semi-annual and annual tides due to the sun's attraction. Happily, however, for our object there is no meteorological or other disturbing cause which produces periodic changes of sea-level in either the fortnightly declinational or the monthly elliptic period; and the lunar gravitational tides in these periods are therefore to be carefully investigated in order that we may obtain the answer to the interesting question, how much does the earth as an elastic spheroid yield to the tide-generating influence of sun or moon? Hitherto in the British Association Committee's reductions of Tidal Observations we have not succeeded in obtaining any trustworthy indications of either of these tides. The St. George's pier landing-stage pontoon was unhappily chosen, for the Liverpool tide gauge cannot be trusted for so delicate an investigation; the available funds for calculation were expended before the long-period tides for Helbre Island could be attacked, and three years of Kurrachee gave our only approach to a result. Comparisons of this, with an indication of a result with calculations on West Hartlepool tides, conducted with the assistance of a grant from the Royal Society, seem to show possibly no sensible yielding, or perhaps, more probably some degree of yielding, of the earth's figure. The absence from all the results of any indication of a 18·6

yearly tide (according to the same law as the other long-period tides) is not easily explained without assuming or admitting a considerable degree of yielding.

Closely connected with the question of the earth's rigidity, and of as great scientific interest and even of greater practical moment, is the question—how nearly accurate is the earth as a time-keeper? and another of, at all events, equal scientific interest—how about the permanence of the earth's axis of rotation?

Peters and Maxwell, about thirty-five and twenty-five years ago, separately raised the question, how much does the earth's axis of rotation deviate from being a principal axis of inertia? and pointed out that an answer to this question is to be obtained by looking for a variation in latitude of any or every place on the earth's surface in a period of 306 days. The model before you illustrates the travelling round of the instantaneous axis relatively to the earth in an approximately circular cone whose axis is the principal axis of inertia, and relatively to space in a cone round a fixed axis. In the model, the former of these cones, fixed relatively to the earth, rolls internally on the latter, supposed to be fixed in space. Peters gave a minute investigation of observations at Pulkova in the years 1841-42, which seemed to indicate at that time a deviation amounting to about $\frac{3}{4}''$ of the axis of rotation from the principal axis. Maxwell, from Greenwich observations of the years 1851–1854, found seeming indications of a very slight deviation—something less than half a second—but differing altogether in phase from that which the deviation indicated by Peters, if real and permanent, would have produced at Maxwell's later time. On my begging Prof. Newcomb to take up the subject, he kindly did so at once, and undertook to analyze a series of observations suitable for the purpose, which had been made in the United States Naval Observatory, Washington. A few weeks later I received from him a letter referring me to a paper by Dr. Nysen, of Pulkova Observatory, in which a similar negative conclusion as to constancy of magnitude or direction in the deviation sought for is arrived at from several series of the Pulkova observations between the years 1842 and 1872, and containing the following statement of his conclusions:—

"The investigation of the ten month period of latitude from the Washington prime vertical observations from 1862 to 1867 is completed, indicating a coefficient too small to be measured with certainty. The declinations with this instrument are subject to an annual period which made it necessary to discuss those of each month separately. As the series extended through a full five years, each month thus fell on five nearly equidistant

points of the period. If x and y represent the co-ordinates of the axis of instantaneous rotation on June 30, 1864, then the observations of the separate months gave the following values of x and y:—

	x	Weight.	y	Weight.
	"		"	
January	− 0·35	10	+ 0·32	
February	− 0·03	14	+ 0·09	
March	+ 0·17	10	+ 0·16	
April	+ 0·44	5	+ 0·05	
May	+ 0·08	16	+ 0·02	
June	− 0·01	14	− 0·01	
July	− 0·05	14	− 0·00	
August	− 0·24	14	+ 0·29	
September	+ 0·18	14	+ 0·21	
October	+ 0·13	14	− 0·01	
November	+ 0·08	17	− 0·20	
December	− 0·08	16	− 0·08	
Mean	0″·01±″·03		+ 0″·05±″·03	

Accepting these results as real they would indicate a radius of rotation of the instantaneous axis amounting, at the earth's surface, to 5 feet and a longitude of the point in which this axis intersects the earth's surface near the north pole such that on July 11, 1864, it was 180° from Washington, or 103° east of Greenwich. The excess of the co-efficient over its probable error is so slight that this result cannot be accepted as anything more than a consequence of the unavoidable errors of observation."

From the discordant character of these results we must not, however, infer that the deviations indicated by Peters, Maxwell, and Newcomb are unreal. On the contrary, any that fall within the limits of probable error of the observations ought properly to be regarded as real. There is in fact a *vera causa* in the temporary changes of sea-level due to meteorological causes, chiefly winds, and to meltings of ice in the polar regions and return evaporations, which seems amply sufficient to account for irregular deviations of from $\frac{1}{2}$″ to $\frac{1}{20}$″ of the earth's instantaneous axis from the axis of maximum inertia, or, as I ought rather to say, of the axis of maximum inertia from the instantaneous axis.

As for geological upheavals and subsidences, if on a very large scale of area, they must produce, on the period and axis of the earth's rotation, effects comparable with those produced by changes of sea-level equal to them in vertical amount. For simplicity, calculating as if the earth were of equal density throughout, I find that an upheaval of all the earth's surface in

north latitude and east longitude, and south latitude and west longitude, with equal depressions in the other two quarters, amounting at greatest to 10 centimeters, and graduating regularly from the points of maximum elevation to the points of maximum depression in the middles of the four quarters, would shift the earth's axis of maximum moment of inertia through 1″ on the north side towards the meridian of 90° W. longitude, and on the south side towards the meridian of 90° E. longitude. If such a change were to take place suddenly, the earth's instantaneous axis would experience a sudden shifting of but $\frac{1}{3}\frac{1}{0}\frac{1}{0}''$ (which we may neglect) and then, relatively to the earth, would commence traveling, in a period of 306 days, round the fresh axis of maximum moment of inertia. The sea would be set into vibration, one ocean up and another down through a few centimeters, like water in a bath set aswing. The period of these vibrations would be from twelve to twenty-four hours, or at most a day or two; their subsidence would probably be so rapid that after at most a few months they would become insensible. Then a regular 306 days' period tide of 11 centimeters from lowest to highest would be to be observed, with gradually diminishing amount from century to century, as though the dissipation of energy produced by this tide, the instantaneous axis of the earth is gradually brought into coincidence with the fresh axis of maximum moment of inertia. If we multiply these figures by 3,600, we find what would be the result of a similar sudden upheaval and subsidence of the earth to the extent of 360 meters above and below previous levels. It is not impossible that in the very early ages of geological history such an action as this, and the consequent 400 meters tide producing a succession of deluges every 306 days for many years may have taken place: but it seems more probable that even in the most ancient times of geological history the great world-wide changes, such as the upheavals of the continents and subsidences of the ocean beds from the general level of their supposed molten origin, took place gradually through the thermodynamic melting of solids and the squeezing out of liquid lava from the interior to which I have already referred. A slow distortion of the earth as a whole would never produce any great angular separation between the instantaneous axis and axis of maximum moment of inertia for the time being. Considering, then, the great facts of the Himalayas and Andes, and Africa and the depths of the Atlantic, and America and the depths of the Pacific, and Australia, and considering further the ellipticity of the equatorial section of the sea-level estimated by Capt. Clarke at about $\frac{1}{1}\frac{1}{0}$ of the mean ellipticity of meridianal sections of the sea-level, we need no brush from the comet's tail, a wholly chimerical cause which can never have

been put forward seriously except in ignorance of elementary dynamical principles, to account for a change in the earth's axis; we need no violent convulsion producing a sudden distortion on a great scale with change of the axis of maximum moment of inertia followed by gigantic deluges; and we may not merely admit, but assert as highly probable, that the axis of maximum inertia and axis of rotation, always very near one another, may have been in ancient times very far from their present geographical position, and may have gradually shifted through ten, twenty, thirty, forty, or more degrees without, at any time, any perceptible sudden disturbance of either land or water.

Lastly, as to variations in the earth's rotational period:—You all, no doubt, know how in 1853 Adams discovered a correction to be needed in the theoretical calculation with which Laplace followed up his brilliant discovery of the dynamical explanation of an apparent acceleration of the moon's mean motion, shown by records of ancient eclipses; and how he found that when his correction was applied, the dynamical theory of the moon's motion accounted for only about half of the observed apparent acceleration; and how Delauney in 1866 verified Adams's result, and suggested that the explanation may be a retardation of the earth's rotation by tidal friction. The conclusion is that since March 19, 721 B. C., a day on which an eclipse of the moon was seen in Babylon, commencing "when one hour after her rising was fully passed," the earth has lost rather more than $\frac{1}{3\,6\,6\,0\,0\,0\,0}$ of her rotational velocity, or as a timekeeper, is going slower by $11\frac{1}{2}$ seconds per annum now than then. According to this rate of retardation, if uniform, the earth at the end of a century would, as a timekeeper, be found 22 seconds behind a perfect clock, rated and set to agree with her at the beginning of the century. Newcomb's subsequent investigations in the lunar theory have on the whole tended to confirm this result, but they have also brought to light some remarkable apparent irregularities in the moon's motion, which, if real, refuse to be accounted for by the gravitational theory without the influence of some unseen body or bodies passing near enough to the moon to influence her mean motion. This hypothesis Newcomb considers not so probable as that the apparent irregularities of the moon are not real and are to be accounted for by irregularities in the earth's rotational velocity. If this is the true explanation it seems that the earth was going slow from 1850 to 1862, so much as to have got behind 7 seconds in these 12 years, and then to have begun going faster again so as to gain 8 seconds from 1862 to 1872. So great an irregularity as this would require somewhat greater changes of sea-level, but not many times greater, than the British Association Commit-

tee's reductions of tidal observations for several places in different parts of the world, allow us to admit to have possibly taken place. The assumption of a fluid interior, which Newcomb suggests, and the flow of a large mass of the fluid "from equatorial regions to a position nearer the axis," is not, from what I have said to you, admissible as a probable explanation of the remarkable acceleration of rotational velocity which seems to have taken place about 1862; but happily it is not necessary. A settlement of 14 centimeters in the equatorial regions with corresponding rise of 28 centimeters at the poles, which is so slight as to be absolutely undiscoverable in astronomical observatories, and which would involve no change of sea-level absolutely disproved by reductions of tidal observations hitherto made would suffice. Such settlements must occur from time to time; and a settlement of the amount suggested might result from the diminution of centrifugal force due to 150 or 200 centuries' tidal retardation of the earth's rotational speed.

ART. LXII.—*Address at the Glasgow Meeting of the British Association, by* ALFRED RUSSEL WALLACE, President of the Section of Biology.

Introduction.

THE range of subjects comprehended within this Section is so wide, and my own acquaintance with them so imperfect and fragmentary, that it is not in my power to lay before you any general outline of the recent progress of the biological sciences. Neither do I feel competent to give you a summary of the present status of any one of the great divisions of our science—such as Anatomy, Physiology, Embryology, Histology, Classification, or Evolution—Philology, Ethnology, or Prehistoric Archæology; but there are fortunately several outlying and more or less neglected subjects to which I have for some time had my attention directed, and which I hope will furnish matter for a few observations, of some interest to biologists, and at the same time not unintelligible to the less scientific members of the Association who may honor us with their presence.

The subjects I first propose to consider have no general name, and are not easily grouped under a single descriptive heading; but they may be compared with that recent development of a sister-science, which has been termed Surface-geology, or Earth-sculpture. In the older geological works we learnt much about strata, and rocks, and fossils, their superposition, contortions, chemical constitution, and affinities, with some general notions of how they are formed in the remote past; but we often came

to the end of the volume no whit the wiser as to how and why the surface of the earth came to be so wonderfully and beautifully diversified; we were not told why some mountains are rounded and others precipitous; why some valleys are wide and open, others narrow and rocky; why rivers so often pierce through mountain-chains; why mountain lakes are often so enormously deep; whence came the gravel, and drift, and erratic blocks, so strangely spread over wide areas while totally absent from the other areas equally extensive. So long as these questions were almost ignored, geology could hardly claim to be a complete science, because, while professing to explain how the crust of the earth came to be what it is, it gave no intelligible account of the varied phenomena presented by its surface. But of late years these surface-phenomena have been assiduously studied; the marvellous effects of denudation and glacial action in giving the final touches to the actual contour of the earth's surface, and their relation to climatic changes and the antiquity of man, have been clearly traced, thus investing geology with a new and popular interest, and at the same time elucidating many of the phenomena presented in the older formations.

Now, just as a surface-geology was required to complete that science, so a surface-biology was wanted to make the science of living things more complete and more generally interesting, by applying the results arrived at by special workers, to the interpretation of those external and prominent features whose endless variety and beauty constitute the charm which attracts us to the contemplation or to the study of nature. We have the descriptive zoologist, for example, who gives us the external characters of animals; the anatomist studies their internal structure; the histologist makes known the nature of their component tissues; the embryologist patiently watches the progress of their development; the systematist groups them into classes and orders, families, genera, and species; while the field-naturalist studies for us their food and habits and general economy. But till quite recently, none of these earnest students, nor all of them combined, could answer satisfactorily, or ever attempted to answer, many of the simplest questions concerning the external characters and general relations of animals and plants. Why are flowers so wonderfully varied in form and color? What causes the Arctic fox and the ptarmigan to turn white in winter? Why are there no elephants in America and no deer in Australia? Why are closely allied species rarely found together? Why are male animals so frequently bright colored? Why are extinct animals so often larger than those which are now living? What has led to the production of the gorgeous train of the peacock and of the two kinds of flower in the primrose? The solution

of these and a hundred other problems of like nature, was rarely approached by the old method of study, or if approached was only the subject of vague speculation. It is to the illustrious author of the "Origin of Species" that we are indebted, for teaching us how to study nature as one great, compact, and beautifully adjusted system. Under the touch of his magic wand the countless isolated facts of internal and external structure of living things—their habits, their colors, their development, their distribution, their geological history,—all fell into their approximate places; and although, from the intricacy of the subject and our very imperfect knowledge of the facts themselves, much still remains uncertain; yet we can no longer doubt that even the minutest and most superficial peculiarities of animals and plants either, on the one hand, are or have been useful to them, or, on the other hand, have been developed under the influence of general laws, which we may one day understand to a much greater extent than we do at present. So great is the alteration effected in our comprehension of nature by the study of variation, inheritance, cross-breeding, competition, distribution, protection, and selection—showing, as they often do, the meaning of the most obscure phenomena, and the mutual dependence of the most widely-separated organisms, that it can only be fitly compared with the analogous alteration produced in our conception of the universe by Newton's grand discovery of the law of gravitation.

I know it will be said (and is said), that Darwin is too highly rated; that some of his theories are wholly and others partially erroneous, and that he often builds a vast superstructure on a very uncertain basis of doubtfully interpreted facts. Now, even admitting this criticism to be well founded- -and I myself believe that to a limited extent it is so—I nevertheless maintain that Darwin is not and cannot be too highly rated. For his greatness does not all depend upon his being infallible, but on his having developed, with rare patience and judgment, a new system of observation and study, guided by certain general principles which are almost as simple as gravitation, and as wide-reaching in their effects. And if other principles should hereafter be discovered, or if it be proved that some of his subsidiary theories are wholly or partially erroneous, this very discovery can only be made by following in Darwin's steps, by adopting the method of research which he has taught us, and by largely using the rich stores of material which he has collected. The "Origin of Species," and the grand series of works which have succeeded it, have revolutionized the study of biology. They have given us new ideas and fertile principles. They have infused life and vigor into our science, and have opened up hitherto unthought of lines of research on which

hundreds of eager students are now laboring. Whatever modifications some of his theories may require, Darwin must none the less be looked up to as the founder of philosophical biology.

As a small contribution to this great subject, I propose now to call your attention to some curious relations of organisms to their environment, which seem to me worthy of more systematic study than has hitherto been given them. The points I shall more especially deal with are—the influence of locality, or of some unknown local causes, in determining the colors of insects and, to a less extent, of birds: and the way in which certain peculiarities in the distribution of plants may have been brought about by their dependence on insects. The latter part of my address will deal with the present state of our knowledge as to the antiquity and early history of mankind.

2. *On some Relations of Living Things to their Environment.*

Of all the external characters of animals, the most beautiful, the most varied, and the most generally attractive, are the brilliant colors and strange yet often elegant markings with which so many of them are adorned. Yet, of all characters, this is the most difficult to bring under the laws of utility or of physical connection. Mr. Darwin—as you are well aware—has shown how wide is the influence of sex on the intensity of coloration; and he has been led to the conclusion that active or voluntary sexual selection is one of the chief causes, if not the chief cause, of all the variety and beauty of color we see among the higher animals. This is one of the points on which there is much divergence of opinion even among the supporters of Mr. Darwin, and one as to which I myself differ from him. I have argued, and still believe, that the need of protection is a far more efficient cause of variation of color than is generally suspected; but there are evidently other causes at work, and one of these seems to be an influence depending strictly on locality, whose nature we cannot yet understand, but whose effects are everywhere to be seen when carefully searched for.

Although the careful experiments of Sir John Lubbock have shown that insects can distinguish colors—as might have been inferred from the brilliant colors of the flowers which are such an attraction to them—yet we can hardly believe that their appreciation and love of distinctive colors is so refined as to guide and regulate their most powerful instinct—that of reproduction. We are therefore led to seek some other cause for the varied colors that prevail among insects; and as this variety is most conspicuous among butterflies,—a group perhaps better known than any other—it offers the best means of studying the subject. The variety of color and marking among these insects is something marvellous. There are probably about ten thousand

different kinds of butterflies now known, and about half of these are so distinct in color and marking that they can be readily distinguished by this means alone. Almost every conceivable tint and pattern is represented, and the hues are often of such intense brilliance and purity as can be equalled by neither birds nor flowers.

Any help to a comprehension of the causes which may have concurred in bringing about so much diversity and beauty must be of value, and this is my excuse for laying before you the more important cases I have met with of a connection between color and locality.

Our first example is from tropical Africa, where we find two unrelated groups of butterflies belonging to two very distinct families (Nymphalidæ and Papilionidæ) characterized by a prevailing blue green color not found in any other continent.* Again, we have a group of African Pieridæ which are white or pale yellow with a marginal row of bead-like black spots, and in the same country one of the Lycænidæ (*Liptena erastus*) is colored so exactly like these that it was at first described as a species of *Pieris*. None of these four groups are known to be in any way specially protected so that the resemblance cannot be due to protective mimicry.

In South America we have far more striking cases. For in the three sub-families— Danainæ, Acræniæ, and Heliconiinæ— all of which are specially protected, we find identical tints and patterns reproduced, often in the greatest detail, each peculiar type of coloration being characteristic of distinct geographical subdivisions of the continent. Nine very distinct genera are implicated in these parallel changes—*Lycorea*, *Ceratinia*, *Mechanitis*, *Ithomia*, *Melinæa*, *Tithorea*, *Acræa*, *Heliconius*, and *Eueides* —groups of three or four (or even of five) of them appearing together in the same livery in one district, while in an adjoining district most or all of them undergo a simultaneous change of coloration or of marking. Thus in the genera *Ithomia*, *Mechanitis*, and *Heliconius*, we have species with yellow apical spots in Guiana, all represented by allied species with white apical spots in South Brazil. In *Mechanitis*, *Melinæa*, and *Heliconius*, and sometimes in *Tithorea*, the species of the Southern Andes (Bolivia and Peru) are characterized by an orange and black livery, while those of the Northern Andes (New Grenada) are almost always orange-yellow and black. Other changes of a like nature, which it would be tedious to enumerate, but which are very striking when specimens are examined, occur in species of the same groups inhabiting these same localities, as well as Central America and the Antilles. The resemblance thus pro-

* *Romaleosoma Euryphene* (Nymphalidæ), *Papilio zalmoxis*, and several species of the *Nireus* group (Papilionidæ).

duced between widely different insects is sometimes general, but often so close and minute that only a critical examination of structure can detect the difference between them. Yet this can hardly be true mimicry, because all are alike protected by the nauseous secretion which renders them unpalatable to birds.

In another series of genera (*Catagramma*, *Callithea*, and *Agrias*), all belonging to the Nymphalidæ, we have the most vivid blue ground, with broad bands of orange-crimson or a different tint of blue or purple, exactly reproduced in corresponding, yet unrelated species, occurring in the same locality; yet, as none of these groups are protected, this can hardly be true mimicry. A few species of two other genera in the same country (*Eunice* and *Siderone*) also reproduce the same colors, but with only a general resemblance in the marking. Yet again, in Tropical America we have species of *Apatura* which, sometimes in both sexes, sometimes in the female only, exactly imitate the peculiar markings of another genus (*Heterochroa*) confined to America. Here, again, neither genus is protected, and the similarity must be due to unknown local causes.

But it is among islands that we find some of the most striking examples of the influence of locality on color, generally in the direction of paler, but sometimes of darker and more brilliant hues, and often accompanied by an unusual increase of size. Thus, in the Moluccas and New Guinea we have several Papilios (*P. euchenor*, *P. ormenus*, and *P. tydeus*), distinguished from their allies by a much paler color, especially in the females, which are almost white. Many species of *Danais* (forming the sub-genus *Iedopsis*) also very pale. But the most curious are the Euplœas, which, in the larger islands, are usually of rich dark colors, while in the small islands of Banda, Ké, and Matabello at least three species not nearly related to each other (*E. Hoppferi*, *E. euripon*, and *E. assimilata*) are all broadley banded or suffused with white, their allies in the larger islands being all very much darker. Again, in the genus *Diadema*, belonging to a distinct family, three species from the small Aru and Ké islands (*D. deois*, *D. Hewitsonii*, and *D. polymena*) are all more conspicuously white-marked than their representatives in the larger islands. In the beautiful genus *Cethosia*, a species from the small island of Waigiou (*C. cyrene*), is the whitest of the genus. *Prothoe* is represented by a blue species in the continental island of Java, while those inhabiting the ancient insular groups of the Moluccas and New Guinea are all pale yellow or white. The genus *Drusilla*, almost confined to these islands, comprises many species which are all very pale; while in the small island of Waigiou is found a very distinct genus, *Hyantis*, which, though differing completely in the neuration of the wings, has exactly the same pale

colors and large ocellated spots as *Drusilla*. Equally remarkable is the fact that the small island of Amboina produces larger-sized butterflies than any of the larger islands which surround it. This is the case with at least a dozen butterflies belonging to many distinct genera,* so that it is impossible to attribute it to other than some local influence. In Celebes, as I have elsewhere pointed out,† we have a peculiar form of wing and much larger size running through a whole series of distinct butterflies, and this seems to take the place of any specialty in color.

From the Fiji Islands we have comparatively few butterflies, but there are several species of *Diadema* of unusually pale colors, some almost white.

The Philipine Islands seem to have the peculiarity of developing metallic colors. We find there at least three species of *Euplœa*‡ not closely related, and all of more intense metallic luster than their allies in other islands. Here also we have one of the large yellow Ornithopteræ (*O. Magellanus*), whose hind wings glow with an intense opaline luster not found in any other species of the entire group; and an *Adolias*§ is larger and of more brilliant metallic coloring than any other species in the Archipelago. In these islands also we find the extensive and wonderful genus of weevils, *Pachyrhynchus*, which in their brilliant metallic coloring surpass anything found in the whole eastern hemisphere, if not in the whole world.

In the Andaman Islands, in the Bay of Bengal, there are a considerable number of peculiar species of butterflies differing slightly from those on the continent, and generally in the direction of paler or more conspicuous coloring. Thus, two species of *Papilio*, which on the continent have the tails black, in their Andaman representatives have them either red or white-tipped.‖ Another species** is richly blue-banded where its allies are black; while three species of distinct genera of Nymphalidæ†† all differ from their allies on the continent in being of excessively pale colors, as well as of somewhat larger size.

In Madagascar we have the very large and singularly white-spotted *Papilio antenor*, while species of three other genera‡‡ are very white or conspicuous, compared with their continental allies.

* *Ornithoptera priamus, O. helena, Papilio deiphobus, P. Ulysses, P. Gambrisuis, P. codrus, Iphias leucippe, Euplœa prothöe, Hestia idea, Athyma jocaste, Diadema pandarus, Nymphalis pyrrhus, N. euryalus, Drusilla jairus.*

† "Contributions to the Theory of Natural Selection," pp. 168–173.

‡ *Euplœa Hewitsonii, E. Diocleliana, E. lætifica, E. dupesnii.*

§ *Adolias calliphorus.*

‖ *Papilio rhodifer* (near *P. Doubledayi*) and *Papilio charicles* (near *P. memnon*).

** *Papilio mayo.*

†† *Euplœa Andamanensis, Cethosia biblis, Cyrestis cocles.*

‡‡ *Danais nossima, Melanitis massoura, Diadema dexithea.*

Passing to the West Indian Islands and Central America (which latter country has formed a group of islands in very recent times), we have similar indications. One of the largest of the Papilios inhabits Jamaica,* while another, the largest of its group, is found in Mexico.† Cuba has two of the same fine genus, whose colors are of surpassing brilliancy;‡ while the genus *Clothilda*—confined to the Antilles and Central America—is remarkable for its rich and showy coloring.

Persons who are not acquainted with the important structural differences that distinguish these various genera of butterflies, can hardly realize the importance and the significance of such facts as I have now detailed. It may be well, therefore, to illustrate them by supposing parallel cases to occur among the mammalia. We might have, for example, in Africa, the gnus, the elands, and the buffaloes all colored and marked like zebras, stripe for stripe over the whole body exactly corresponding. So the hares, marmots, and squirrels of Europe might be all red, with black feet, while the corresponding species of Central Asia were all yellow, with black heads. In North America we might have raccoons, squirrels, and opossums in parti-colored livery of white and black, so as exactly to resemble the skunk of the same country; while in South America they might be black, with a yellow throat patch, so as to resemble with equal closeness the tayra of the Brazilian forests. Were such resemblances to occur in anything like the number, and with the wonderful accuracy of imitation met with among the Lepidoptera, they would certainly attract universal attention among naturalists, and would lead to the exhaustive study of the influence of local causes in producing such startling results.

One somewhat similar case does indeed occur among the mammalia, two singular African animals, the Aard-wolf (*Proteles*) and the Hyæna-dog (*Lycaon*), both strikingly resembling hyænas in their general form as well as in their spotted markings. Belonging as they all do to the Carnivora, though to three distinct families, it seems quite an analogous case to those we have imagined; but as the Aard-wolf and the hyæna-dog are both weak animals compared with the hyæna, the resemblance may be useful, and in that case would come under the head of mimicry. This seems the more probable because, as a rule, the colors of the Mammalia are protective, and are too little varied to allow of the influence of local causes producing any well-marked effects.

When we come to the birds, however, the case is different; for although they do not exhibit such distinct marks of the influence of locality as do butterflies—probably because the causes

* *Papilio Homerus.* † *P. daunus.* ‡ *P. Gundlachianus, P. Villiersi.*

which determine color are in their case more complex—yet there are distinct indications of some effect of the kind, and we must devote some little time to their consideration.

One of the most curious cases is that of the parrots of the West Indian Islands and Central America, several of which have white heads or foreheads, occurring in two distinct genera,* while none of the more numerous parrots of South America are so colored. In the small island of Dominica we have a very large and richly-colored parrot (*Chrysotis augusta*) corresponding to the large and richly-colored *Papilio homerus* of Jamaica.

The Andaman Islands are equally remarkable, at least six of the peculiar birds differing from their continental allies in being much lighter, and sometimes with a large quantity of pure white in the plumage,† exactly corresponding to what occurs among the butterflies.

In the Philipines this is not so marked a feature,—yet we have here the only known white-breasted Kingcrow (*Dicrurus mirabilis*),—the newly discovered *Eurylæmus Steerii*, wholly white beneath,—three species of *Diceum*, all white beneath,—several species of *Parus*, largely white-spotted,—while many of the pigeons have light ashy tints. The birds generally, however, have rich-dark colors, similar to those which prevail among the butterflies.

In Celebes we have a swallow-shrike and a peculiar small crow allied to the jackdaw,‡ whiter than any of their allies in the surrounding islands, but otherwise the colors of the birds call for no special remark.

In Timor and Flores we have white-headed pigeons,§ and a long-tailed flycatcher almost entirely white.‖

In the small Lord Howe's Island we have the recently extinct white rail (*Notornis alba*), remarkably contrasting with its allies in the larger islands in New Zealand.

We cannot, however lay any stress on isolated examples of white colors, since these occur in most of the great continents, but where we find a series of species of distinct genera, all differing from their continental allies in a whiter coloration, as in the Andaman Islands and the West Indies; and among butterflies, in the smaller Moluccas, the Andamans, and Madagascar, we cannot avoid the conclusion that in these insular localities some general cause is at work.

There are other cases, however, in which local influences seem to favor the production of preservation of intense crimson

* *Pionus albifrons* and *Chrysotis senilis* (C. America), *Chrysotis Sallæi* (Hayti).

† *Kittacincla albiventris, Geocichla albigularis, Sturnia Andamanensis, Hyloterpe grisola, var., Fanthœnas palumboides, Osmotreron chloroptera.*

‡ *Artamus monachus, Corvus advena.*

§ *Ptilopus cinctus, P. albocinctus.* ‖ *Tchiterea affinis, var.*

or a very dark coloration. Thus in the Moluccas and New Guinea alone we have bright red parrots belonging to two distinct families,* and which, therefore, most probably have been independently produced or preserved by some common cause. Here too and in Australia we have black parrots and pigeons;† and it is a most curious and suggestive fact that in another insular sub-region—that of Madagascar and the Mascarene Islands—these same colors reappear in the same two groups.‡

Some very curious physiological facts bearing upon the presence or absence of white colors in the higher animals have lately been adduced by Dr. Ogle.§ It has been found that a colored or dark pigment in the olfactory region of the nostrils is essential to perfect smell, and this pigment is rarely deficient except when the whole animal is purely white. In these cases the creature is almost without smell or taste. This, Dr. Ogle believes, explains the curious case of the pigs in Virginia adduced by Mr. Darwin, white pigs being poisoned by a poisonous root which does not affect black pigs. Mr. Darwin imputed this to a constitutional difference accompanying the dark color, which rendered what was poisonous to the white-colored animals quite innocuous to the black. Dr. Ogle, however, observes, that there is no proof that the black pigs eat the root, and he believes the more probable explanation to be that it is distasteful to them, while the white pigs, being deficient in smell and taste, eat it and are killed. Analogous facts occur in several distinct families. White sheep are killed in the Tarentino by eating *Hypericum crispum*, while black sheep escape; white rhinoceroses are said to perish from eating *Euphorbia candelabrum;* and white horses are said to suffer from poisonous food where colored ones escape. Now it is very improbable that a constitutional immunity from poisoning by so many distinct plants should in the case of such widely different animals be always correlated with the same difference of color; but the facts are readily understood if the senses of smell and taste are dependent on the presence of a pigment which is deficient in wholly white animals. The explanation has however, been carried a step further, by experiments showing that the absorption of odor by dead matter, such as clothing, is greatly affected by color, black being the most powerful absorbent, then blue, red, yellow, and lastly white. We have here a physical cause for the sense-inferiority of totally white animals which may account for their rarity in nature. For few, if any, wild animals are wholly white. The head, the face, or

* *Lorius*, *Eos* (Trichoglossidæ), *Eclectus* (Palæornithidæ).
† *Microglossus*, *Calyptorhynchus*, *Turacœna*.
‡ *Coracopsis*, *Alectrœnas*.
§ Medico-Chirurgical Transactions, vol. liii, (1870).

at least the muzzle or the nose, are generally black. The ears and eyes are also often black; and there is reason to believe that dark pigment is essential to good hearing, as it certainly is to perfect vision. We can therefore understand why white cats with blue eyes are so often deaf—a peculiarity we notice more readily than their deficiency of smell or taste.

If then the prevalence of white coloration is generally accompanied with some deficiency in the acuteness of the most important senses, the color becomes doubly dangerous, for it not only renders its possessors more conspicuous to its enemies, but at the same time makes it less ready in detecting the presence of danger. Hence, perhaps, the reason why white appears more frequently in islands where competition is less severe and enemies less numerous and varied. Hence, also, a reason why *albinoism*, although freely occurring in captivity never maintains itself in a wild state, while *melanism* does. The peculiarity of some islands in having all their inhabitants of dusky colors—as the Galapagos—may also perhaps be explained on the same principles, for poisonous fruits or seeds may there abound which weed out all white or light-colored varieties, owing to their deficiency of smell and taste. We can hardly believe, however, that this would apply to white-colored butterflies, and this may be a reason why the effect of an insular habitat is more marked in these insects than in birds or mammals. But though inapplicable to the lower animals, this curious relation of sense-acuteness with colors may have had some influence on the development of the higher human races. If light tints of the skin were generally accompanied by some deficiency in the sense of smell, hearing, and vision, the white could never compete with the darker races, so long as man was in a very low or savage condition, and wholly dependent for existence on the acuteness of his senses. But as the mental faculties became more fully developed and more important to his welfare than mere sense-acuteness, the lighter tints of skin, and hair, and eyes, would cease to be disadvantageous whenever they were accompanied by superior brain-power. Such variations would then be preserved; and thus may have arisen the Xanthochroic race of mankind, in which we find a high development of intellect accompanied by a slight deficiency in the acuteness of the senses as compared with the darker forms.

I have now to ask your attention to a few remarks on the peculiar relations of plants and insects as exhibited in islands.

Ever since Mr. Darwin showed the immense importance of insects in the fertilization of flowers, great attention has been paid to the subject, and the relation of these two very different classes of natural objects has been found to be more universal

and more complex than could have been anticipated. Whole genera and families of plants have been so modified, as first to attract, and then to be fertilized by, certain groups of insects, and this special adaptation seems in many cases to have determined the more or less wide range of the plants in question. It is also known that some species of plants can be fertilized only by particular species of insects, and the absence of these from any locality would necessarily prevent the continued existence of the plant in that area. Here, I believe, will be found the clue to much of the peculiarity of the floras of oceanic islands, since the methods by which they have been stocked with plants and insects will be often quite different. Many seeds are, no doubt, carried by oceanic currents, others probably by aquatic birds. Mr. H. N. Moseley informs me that the albatrosses, gulls, puffins, tropic birds, and many others, nest inland, often amidst dense vegetation, and he believes they often carry seeds, attached to their feathers, from island to island for great distances. In the tropics they often nest on the mountains far inland, and may thus aid in the distribution even of mountain plants. Insects, on the other hand, are mostly conveyed by aërial currents, especially by violent gales; and it may thus often happen that totally unrelated plants and insects may be brought together, in which case the former must often perish for want of suitable insects to fertilize them. This will, I think, account for the strangely fragmentary nature of these insular floras, and the great distances that often exist between those which are situated in the same ocean, as well as for the preponderance of certain orders and genera. In Mr. Pickering's valuable work on the Geographical Distribution of Animals and Plants, he gives a list of no less than sixty-six natural orders of plants *unexpectedly* absent from Tahiti, or which occur in many of the surrounding lands, some being abundant in other islands—as the Labiatæ at the Sandwich Islands. In these latter islands the flora is much richer, yet a large number of families which abound in other parts of Polynesia are totally wanting. Now much of the poverty and exceptional distribution of the plants of these islands is probably due to the great scarcity of flower-frequenting insects. Lepidoptera and Hymenoptera are exceedingly scarce in the eastern islands of the Pacific, and it is almost certain that many plants which require these insects for their fertilization have been thereby prevented from establishing themselves. In the Western islands, such as the Fijis, several species of butterflies occur in tolerable abundance, and no doubt some flower-haunting Hymenoptera accompany them, and in these islands the flora appears to be much more varied, and especially to be characterized by a much greater variety of showy flowers,

as may be seen by examining the plates of Dr. Seeman's "Flora Vitiensis."

Darwin and Pickering both speak of the great preponderance of ferns at Tahiti, and Mr. Moseley, who spent several days in the interior of the island, informs me that "at an elevation of from 2,000 to 3,000 feet the dense vegetation is composed almost entirely of ferns. A tree fern (*Alsophila Tahitensis*) forms a sort of forest, to the exclusion of almost every other tree, and, with huge plants of two other ferns (*Angiopteris evecta* and *Aspelenium nidus*), forms the main mass of the vegetation." And he adds, "I have nowhere seen ferns in so great proportionate abundance." This unusual proportion of ferns is a general feature of insular as compared with continental floras; but it has, I believe, been generally attributed to favorable conditions, especially to equable climate and perennial moisture. In this respect, however, Tahiti can hardly differ greatly from many other islands, which yet have no such vast preponderance of ferns. This is a question that cannot be decided by mere lists of species, since it is probable that in Tahiti they are less numerous than in some other islands where they form a far less conspicuous feature in the vegetation. The island most comparable with Tahiti in that respect is Juan Fernandez. Mr. Moseley writes to me--"In a general view of any wide stretch of densely-clothed mountainous surface of the island, the ferns, both tree-ferns and the unstemmed forms, are seen at once to compose a very large proportion of the mass of foliage." As to the insects of Juan Fernandez, Mr. Edwyn C. Reed, who made two visits and spent several weeks there, has kindly furnished me with some exact information. Of butterflies there is only one (*Pyrameis carie*), and that rare—a Chilian species, and probably an accidental straggler. Four species of moths of moderate size were observed—all Chilian, and a few larvæ and pupæ. Of bees there were none, except one very minute species (allied to *Chilicola*), and of other Hymenoptera, a single specimen of *Ophion luteus*--a cosmopolitan ichneumon. About twenty species of flies were observed, and these formed the most prominent feature of the entomology of the island.

Now, as far as we know, the extreme entomological poverty agrees closely with that of Tahiti; and there are probably no other portions of the globe equally favored in soil and climate and with an equally luxuriant vegetation, where insect-life is so scantily developed. It is curious therefore to find that these two islands also agree in the wonderful predominance of ferns over the flowering plants—in individuals even more than in species, and there is no difficulty in connecting the two facts. The excessive minuteness and great abundance of fern-spores causes them to be far more easily distributed by winds than the

seeds of flowering plants, and they are thus always ready to occupy any vacant places in suitable localities, and to compete with the less vigorous flowering plants. But where insects are so scarce, all plants which require insect fertilization, whether constantly to enable them to produce seed at all, or occasionally to keep up their constitutional vigor by crossing, must be at a great disadvantage; and thus the scanty flora which oceanic islands must always possess, peopled as they usually are by waifs and strays from other lands, is rendered still more scanty by the weeding out of all such as depend largely on insect fertilization for their full development. It seems probable, therefore, that the preponderance of ferns in islands (considered in mass of individuals rather than in number of species) is largely due to the absence of competing phenogamous plants; and that this is in great part due to the scarcity of insects. In other oceanic islands, such as New Zealand and the Galapagos, where ferns, although tolerably abundant, form no such predominant feature in the vegetation, but where the scarcity of flower-haunting insects is almost equally marked, we find a great preponderance of small green, or otherwise inconspicuous flowers, indicating that only such plants have been enabled to flourish there as are independent of insect fertilization. In the Galapagos—which are perhaps even more deficient in flying insects than Juan Fernandez—this is so striking a feature that Mr. Darwin speaks of the vegetation as consisting in great part of "wretched-looking weeds," and states that "it was some time before he discovered that almost every plant was in flower at the time of his visit." He also says that he "did not see one beautiful flower" in the islands. It appears, however, that Compositæ, Leguminosæ, Rubiaceæ, and Solanaceæ, form a large proportion of the flowering plants, and as these are orders which usually require insect fertilization, we must suppose either that they have become modified so as to be self-fertilized, or that they are fertilized by the visits of the minute Diptera and Hymenoptera, which are the only insects recorded from these islands.

In Juan Fernandez, on the other hand, there is no such total deficiency of showy flowers. I am informed by Mr. Moseley that a variety of the Magnoliaceous winter's bark abounds, and has showy white flowers, and that a Bignoniaceous shrub with abundance of dark blue flowers, was also plentiful; while a white-flowered Liliaceous plant formed large patches on the hillsides. Besides these there were two species of woody Compositæ with conspicuous heads of yellow blossoms, and a species of white-flowered myrtle also abundant; so that, on the whole, flowers formed a rather conspicuous feature in the aspect of the vegetation of Juan Fernandez.

But this fact—which at first sight seems entirely at variance

with the view we are upholding of the important relation between the distribution of insects and plants—is well explained by the existence of two species of humming birds in Juan Fernandez, which, in their visits to these large and showy flowers fertilize them as effectually as bees, moths, or butterflies. Mr. Moseley informs me that "these humming birds are *extraordinarily abundant*, every tree or bush having one or two darting about it." He also observed that "nearly all the specimens killed had the feathers round the base of the bill and front of the head clogged and colored yellow with pollen." Here, then, we have the clue to the perpetuation of large and showy flowers in Juan Fernandez; while the total absence of humming-birds in the Galapagos may explain why no such large-flowered plants have been able to establish themselves in those equatorial islands.

This leads to the observation that many other groups of birds also, no doubt, aid in the fertilization of flowers. I have often observed the beaks and faces of the brush-tongued lories of the Moluccas covered with pollen; and Mr. Moseley noted the same fact in a species of *Artamus*, or swallow-shrike, shot at Cape York, showing that this genus also frequents flowers and aids in their fertilization. In the Australian region we have the immense group of the Meliphagidæ, which all frequent flowers, and as these range over all the islands of the Pacific, their presence will account for a certain proportion of showy flowers being found there, such as the scarlet *Metrosideros*, one of the few conspicuous flowers in Tahiti. In the Sandwich Islands, too, there are forests of *Metrosideros*; and Mr. Charles Pickering writes me, that they are visited by honey-sucking birds, one of which is captured by sweetened bird-lime, against which it thrusts its extensile tongue. I am also informed that a considerable number of flowers are occasionally fertilized by humming-birds in North America; so that there can, I think, be little doubt that birds play a much more important part in this respect than has hitherto been imagined. It is not improbable that in Tropical America, where this family is so enormously developed, many flowers will be found to be expressly adapted to fertilization by them just as so many in our own country are specially adapted to the visits of certain families or genera of insects.

It must also be remembered, as Mr. Moseley has suggested to me, that a flower which had acquired a brilliant color to attract insects might, on transference to another country, and becoming so modified as to be capable of self-fertilization, retain the colored petals for an indefinite period. Such is probably the explanation of the *Pelargonium* of Kerguelen's land, which forms masses of bright color near the shore during the flower-

ing season; while most of the other plants of the island have colorless flowers in accordance with the almost total absence of winged insects. The presence of many large and showy flowers among the indigenous flora of St. Helena must be an example of a similar persistence. Mr. Melliss indeed states it to be "a remarkable peculiarity that the indigenous flowers are, with very slight exceptions, all perfectly colorless;"* but although this may apply to the general aspect of the remains of the indigenous flora, it is evidently not the case as regards the *species*, since the interesting plates of Mr. Melliss's volume show that about one-third of the indigenous flowering plants have more or less colored or conspicuous flowers, while several of them are exceedingly showy and beautiful. Among these are a *Lobelia*, three *Wahlenbergias*, several *Compositæ*, and especially the handsome red flowers of the now almost extinct forest-trees, the ebony and redwood (species of *Melhania*, Byttneriaceæ). We have every reason to believe, however, that when St. Helena was covered with luxuriant forests, and especially at that remote period when it was much more extensive than it is now, it must have supported a certain number of indigenous birds and insects, which would have aided in the fertilization of these gaily-colored flowers. The researches of Dr. Hermann Müller have shown us by what minute modification of structure or of function many flowers are adapted for partial insect and self-fertilization in varying degrees, so that we have no difficulty in understanding how, as the insects diminished and finally disappeared, self-fertilization may have become the rule, while the large and showy corollas remain to tell us plainly of a once different state of things.

Another interesting fact in connection with this subject is the presence of arborescent forms of Compositæ in so many of the remotest oceanic islands. They occur in the Galapagos, in Juan Fernandez, in St. Helena, in the Sandwich Islands, and in New Zealand; but they are not directly related to each other, representatives of totally different tribes of this extensive order becoming arborescent in each group of islands. The immense range and almost universal distribution of the Compositæ is due to the combination of a great facility of distribution (by their seeds), with a great attractiveness to insects, and the capacity of being fertilized by a variety of species of all orders, and especially by flies and small beetles. Thus they would be among the earliest of flowering plants to establish themselves on oceanic islands; but where insects of all kinds were very scarce it would be an advantage to gain increased size and longevity, so that fertilization at an interval of several years might suffice for the continuance of the species. The arborescent form would com-

* Melliss's *St. Helena*, p. 226, note.

bine with increased longevity the advantage of increased size in the struggle for existence with the ferns and other early colonists, and these advantages have led to its being independently produced in so many distant localities, whose chief feature in common is their remoteness from continents and the extreme poverty of their insect life.

As the sweet odors of flowers are known to act in combination with their colors, as an attraction to insects, it might be anticipated that where color was deficient scent would be so also. On applying to my friend Dr. Hooker for information as to New Zealand plants, he informed me that this was certainly the case, and that the New Zealand flora is, speaking generally, as strikingly deficient in sweet odors as in conspicuous colors. Whether this peculiarity occurs in other islands I have not been able to obtain information, but we may certainly expect it to be so in such a marked instance as that of the Galapagos flora.

Another question which here comes before us is the origin and meaning of the odoriferous glands of leaves. Dr. Hooker informed me that not only are New Zealand plants deficient in scented flowers, but equally so in scented leaves. This led me to think that perhaps such leaves were in some way an additional attraction to insects, though it is not easy to understand how this could be, except by adding a general attraction to the special attraction of the flowers, or by supporting the larvæ, which as perfect insects aid in fertilization. Mr. Darwin, however, informs me that he considers that leaf-glands bearing essential oils are a protection against the attacks of insects where these abound, and would thus not be required in countries where insects were very scarce. But it seems opposed to this view that highly aromatic plants are characteristic of deserts all over the world, and in such places insects are not abundant. Mr. Stainton informs me that the aromatic Labiatæ enjoy no immunity from insect attacks. The bitter leaves of the cherry-laurel are often eaten by the larvæ of moths that abound on our fruit-trees; while in the Tropics the leaves of the orange tribe are favorites with a large number of lepidopterous larvæ; and our northern firs and pines, although abounding in a highly aromated resin, are very subject to the attacks of beetles. My friend Dr. Richard Spruce—who while traveling in South America allowed nothing connected with plant-life to escape his observation—informs me that trees whose leaves have aromatic and often resinous secretions in immersed glands abound in the plains of tropical America, and that such are in great part, if not wholly, free from the attacks of leaf-eating ants, except where the secretion is only slightly bitter, as in the orange tribe, orange-trees being sometimes entirely denuded

of their leaves in a single night. Aromatic plants abound in the Andes up to about 13,000 feet, as well as in the plains, but hardly more so than in Central and Southern Europe. They are perhaps most plentiful in the dry mountainous parts of Southern Europe; and as neither here nor in the Andes do leaf-eating ants exist, Dr. Spruce infers that, although in the hot American forests where such ants swarm the oil-bearing glands serve as a protection, yet they were not originally acquired for that purpose. Near the limits of perpetual snow on the Andes such plants as occur are not, so far as Dr. Spruce has observed, aromatic; and as plants in such situations can hardly depend on insect visits for their fertilization, the fact is comparable with that of the flora of New Zealand, and would seem to imply some relation between the two phenomena, though what it exactly is cannot yet be determined.

I trust I have now been able to show you that there are a number of curious problems lying as it were on the outskirts of biological inquiry which well merit attention, and which may lead to valuable results. But these problems are, as you see, for the most part connected with questions of locality, and require full and accurate knowledge of the productions of a number of small islands and other limited areas, and the means of comparing them the one with the other. To make such comparisons is, however, now quite impossible. No museum contains any fair representation of the productions of these localities, and such specimens as do exist, being scattered through the general collection, are almost useless for this special purpose. If, then, we are to make any progress in this inquiry, it is absolutely essential that some collectors should begin to arrange their cabinets primarily on a geographical basis, keeping together the productions of every island or group of islands, and of such divisions of each continent as are found to possess any special or characteristic fauna or flora. We shall then be sure to detect many unsuspected relations between the animals and plants of certain localities, and we shall become much better acquainted with those complex reactions between the vegetable and animal kingdoms, and between the organic world and the inorganic, which have almost certainly played an important part in determining many of the most conspicuous features of living things.

3. *Rise and Progress of Modern Views as to the Antiquity and Origin of Man.*

I now come to a branch of our subject which I would gladly have avoided touching on, but as the higher powers of this Association have decreed that I should preside over the Anthropological Department, it seems proper that I should devote

some portion of my address to matters more immediately connected with the special study to which that department is devoted.

As my own knowledge of, and interest in, Anthropology, is confined to the great outlines, rather than to the special details of the science, I propose to give a very brief and general sketch of the modern doctrine as to the Antiquity and Origin of Man, and to suggest certain points of difficulty which have not, I think, yet received sufficient attention.

Many now present remember the time (for it is a little more than twenty years ago) when the antiquity of man, as now understood, was universally discredited. Not only theologians, but even geologists, then taught us that man belonged altogether to the existing state of things; that the extinct animals of the Tertiary period had finally disappeared, and that the earth's surface had assumed its present condition before the human race first came into existence. So prepossessed were even scientific men with this idea—which yet rested on purely negative evidence, and could not be supported by any arguments of scientific value—that numerous facts which had been presented at intervals for half a century, all tending to prove the existence of man at very remote epochs, were silently ignored; and, more than this, the detailed statements of three distinct and careful observers were rejected by a great scientific society as too improbable for publication, only because they proved (if they were true) the co-existence of man with extinct animals!*

But this state of belief in opposition to facts could not long continue. In 1859 a few of our most eminent geologists examined for themselves into the alleged occurrence of flint implements in the gravels of the North of France, which had been made public fourteen years before, and found them strictly correct. The caverns of Devonshire were about the same time carefully examined by equally eminent observers, and were found fully to bear out the statement of those who had published their results eighteen years before. Flint implements began to be found in all suitable localities in the South of England, when carefully searched for, often in gravels of equal antiquity with those of France. Caverns, giving evidence of human occupation at various remote periods, were explored in Belgium and the South of France,—lake dwellings were examined in Switzerland—refuse heaps in Denmark—and thus a whole series of remains have been discovered carrying back the history of mankind from the earliest historic periods to a

* In 1854 (?) a communication from the Torquay Natural History Society confirming previous accounts by Mr. Godwin-Austen, Mr. Vivian, and the Rev. Mr. McEnery, that worked flints occurred in Kent's Hole with remains of extinct species, was rejected as too improbable for publication.

long distant past. The antiquity of the races thus discovered can only be generally determined by the successively earlier and earlier stages through which we can trace them. As we go back, metals soon disappear and we find only tools and weapons of stone and of bone. The stone weapons get ruder and ruder; pottery, and then the bone implements, cease to occur; and in the earliest stage we find only chipped flints, of rude design though still of unmistakable human workmanship. In like manner domestic animals disappear as we go backward; and though the dog seems to have been the earliest, it is doubtful whether the makers of the ruder flint implements of the gravels possessed even this. Still more important as a measure of time are the changes of the earth's surface—of the distribution of animals—and of climate—which have occurred during the human period. At a comparatively recent epoch in the record of prehistoric times we find that the Baltic was far salter than it is now, and produced abundance of oysters; and that Denmark was covered with pine forests inhabited by Capercailzies, such as now only occur further north in Norway. A little earlier we find that Reindeer were common even in the South of France, and still earlier this animal was accompanied by the mammoth and woolly rhinoceros, by the arctic glutton, and by huge bears and lions of extinct species. The presence of such animals implies a change of climate, and both in the caves and gravels we find proofs of a much colder climate than now prevails in Western Europe. Still more remarkable are the changes of the earth's surface which have been effected during man's occupation of it. Many extensive valleys in England and France are believed by the best observers to have been deepened at least a hundred feet; caverns now far out of the reach of any stream must for a long succession of years have had streams flowing through them, at least in times of floods—and this often implies that vast masses of solid rock have since been worn away. In Sardinia land has risen at least 300 feet since men lived there who made pottery and probably used fishing-nets;* while in Kent's Cavern remains of man are found buried beneath two separate beds of stalagmite, each having a distinct texture, and each covering a deposit of cave-earth having well-marked differential characters, while each contains a distinct assemblage of extinct animals.

Such, briefly, are the results of the evidence that has been rapidly accumulating for about fifteen years as to the antiquity of man; and it has been confirmed by so many discoveries of a like nature in all parts of the globe, and especially by the comparison of the tools and weapons of prehistoric man with those of modern savages, so that the use of even the rudest flint im-

* Lyell's Antiquity of Man, fourth edition, p. 115.

plements has become quite intelligible,—that we can hardly wonder at the vast revolution effected in public opinion. Not only is the belief in man's vast and still unknown antiquity universal among men of science, but it is hardly disputed by any well-informed theologian; and the present generation of science-students must, we should think, be somewhat puzzled to understand, what there was in the earlier discoveries that should have aroused such general opposition and been met with such universal incredulity.

But the question of the mere "Antiquity of Man" almost sank into insignificance at a very early period of the inquiry, in comparison with the far more momentuous and more exciting problem of the development of man from some lower animal form, which the theories of Mr. Darwin and of Mr. Herbert Spencer soon showed to be inseparably bound up with it. This has been, and to some extent still is, the subject of fierce conflict; but the controversy as to the fact of such development is now almost at an end, since one of the most talented representatives of Catholic theology, and an anatomist of high standing —Professor Mivart—fully adopts it as regards physical structure, reserving his opposition for those parts of his theory, which would deduce man's whole intellectual and moral nature from the same source, and by a similar mode of development.

Never, perhaps, in the whole history of science or philosophy has so great a revolution in thought and opinion been effected as in the twelve years from 1859 to 1871, the respective dates of publication of Mr. Darwin's "Origin of Species" and "Descent of Man." Up to the commencement of this period the belief in the independent creation or origin of the species of animals and plants, and the very recent appearance of man upon the earth, were, practically, universal. Long before the end of it these two beliefs had utterly disappeared, not only in the scientific world, but almost equally so among the literary and educated classes generally. The belief in the independent origin of man held its ground somewhat longer, but the publication of Mr. Darwin's great work gave even that its deathblow, for hardly anyone capable of judging of the evidence now doubts the derivative nature of man's bodily structure as a whole, although many believe that his mind and even some of his physical characteristics may be due to the action of other forces than have acted in the case of the lower animals.

We need hardly be surprised, under these circumstances, if there has been a tendency among men of science to pass from one extreme to the other, from a profession (so few years ago) of total ignorance as to the mode of origin of all living things, to a claim to almost complete knowledge, of the whole progress of the universe, from the first speck of living protoplasm up to

the highest development of the human intellect. Yet this is really what we have seen in the last sixteen years. Formerly difficulties were exaggerated, and it was asserted that we had not sufficient knowledge to venture on any generalizations on the subject. Now difficulties are set aside, and it is held that our theories are so well established and so far-reaching, that they explain and comprehend all nature. It is not long ago (as I have already reminded you) since *facts* were contemptuously ignored, because they favored our now popular views; at the present day it seems to me that facts which oppose them hardly receive due consideration. And as opposition to the best incentive to progress, and it is not well even for the best theories to have it all their own way, I propose to direct your attention to a few such facts, and to the conclusion that seems fairly deducible from them.

It is a curious circumstance, that notwithstanding the attention that has been directed to the subject in every part of the world, and the numerous excavations connected with railways and mines which have offered such facilities for geological discovery, no advance whatever has been made for a considerable number of years, in detecting the time or the mode of man's origin. The Palæolithic flint weapons first discovered in the North of France more than thirty years ago, are still the oldest undisputed proofs of man's existence; and amid the countless relics of a former world that have been brought to light, no evidence of any one of the links that must have connected man with the lower animals has yet appeared.

It is, indeed, well known that negative evidence in geology is of very slender value, and this is, no doubt, generally the case. The circumstances here are, however, peculiar, for many converging lines of evidence show that on the theory of development by the same laws which have determined the development of the lower animals, man must be immensely older than any traces of him yet discovered. As this is a point of great interest we must devote a few moments to its consideration.

1. The most important difference between man and such of the lower animals as most nearly approach him, is undoubtedly in the bulk and development of his brain, as indicated by the form and capacity of the cranium. We should therefore anticipate that these earliest races, who were contemporary with the extinct animals and used rude stone weapons, would show a marked deficiency in this respect. Yet the oldest known crania—those of the Engis and Cro-Magnon caves—show no marks of degradation. The former does not present so low a type as that of most existing savages, but is—to use the words of Prof. Huxley—"a fair average human skull, which might have belonged to a philosopher, or might have contained the

thoughtless brains of a savage." The latter are still more remarkable, being unusually large and well-formed. Dr. Pruner-Bey states that they surpass the average of modern European skulls in capacity, while their symmetrical forms, without any trace of prognathism, compares favorably not only with the foremost savage races, but with many civilized nations of modern times.

One or two other crania of much lower type, but of less antiquity than this, have been discovered; but they in no way invalidate the conclusion which so highly developed a form at so early a period implies, viz., that we have as yet made a hardly perceptible step toward the discovery of any earlier stage in the development of man.

2. This conclusion is supported and enforced by the nature of many of the works of art found even in the oldest cave-dwellings. The flints are of the old chipped type, but they are formed into a large variety of tools and weapons—such as scrapers, awls, hammers, saws, lances, &c., implying a variety of purposes for which these were used, and a corresponding degree of mental activity and civilization. Numerous articles of bone have also been found, including well-formed needles, implying that skins were sewn together, and perhaps even textile materials woven into cloth. Still more important are numerous carvings and drawings representing a variety of animals, including horses, reindeer, and even a mammoth, executed with considerable skill on bone, reindeer-horns, and mammoth-tusks. These, taken together, indicate a state of civilization much higher than that of the lowest of our modern savages, while it is quite compatible with a considerable degree of mental advancement, and leads us to believe that the crania of Engis and Cro-Magnon are not exceptional, but fairly represent the characters of the race. If we further remember that these people lived in Europe under the unfavorable conditions of a sub-Arctic climate, we shall be inclined to agree with Dr. Daniel Wilson, that it is far easier to produce evidences of deterioration than of progress in instituting a comparison between the contemporaries of the mammoth and later prehistoric races of Europe or savage nations of modern times.*

3. Yet another important line of evidence as to the extreme antiquity of the human type has been brought prominently forward by Prof. Mivart.† He shows by a careful comparison of all parts of the structure of the body, that man is related, not to any one, but almost equally to many of the existing apes—to the orang, the chimpanzee, the gorilla, and even to the gibbons—in a variety of ways; and these relations and differ-

* "Prehistoric Man," 3d ed., vol. i, p. 117.

† "Man and Apes," pp. 171–193.

ences are so numerous and so diverse that on the theory of evolution the ancestral form which ultimately developed into man must have diverged from the common stock whence all these various forms and their extinct allies originated. But so far back as the Miocene deposits of Europe, we find the remains of apes allied to these various forms, and especially to the gibbons, so that in all probability the special line of variation which led up to man branched off at a still earlier period. And these early forms, being the initiation of a far higher type, and having to develop by natural selection into so specialized and altogether distinct a creature as man, must have risen at a very early period into the position of a dominant race, and spread in dense waves of population over all suitable portions of the great continent--for this, on Mr. Darwin's hypothesis, is essential to rapid developmental progress through the agency of natural selection.

Under these circumstances we might certainly expect to find some relics of these earlier forms of man along with those of animals which were presumably less abundant. Negative evidence of this kind is not very weighty, but still it has *some* value. It has been suggested that as apes are mostly tropical, and the anthropoid apes are now confined almost exclusively to the vicinity of the equator, we should expect the ancestral forms also to have inhabited these same localities—West Africa and the Malay islands. But this objection is hardly valid, because existing anthropoid apes are wholly dependent on a perennial supply of easily accessible fruits, which is only found near the equator, while not only had the south of Europe an almost tropical climate in Miocene times, but we must suppose even the earliest ancestors of man to have been terrestrial and omnivorous, since it must have taken ages of slow modification to have produced the perfectly erect form, the short arms, and the wholly non-prehensile foot, which so strongly differentiate man from the apes.

The conclusion which I think we must arrive at is, that if man has been developed from a common ancestor, with all existing apes, *and by no other agencies than such as have affected their development*, then he must have existed in something approaching his present form, during the tertiary period—and not merely existed, but predominated in numbers, wherever suitable conditions prevail. If then continued researches in all parts of Europe and Asia fail to bring to light any proofs of his presence, it will be at least a presumption that he came into existence at a much later date, and by a much more rapid process of development. In that case it will be a fair argument, that, just as he is in his mental and moral nature, his capacities and aspirations, so infinitely raised above the brutes, so his

origin is due to distinct and higher agencies than such as have affected their development.

There is yet another line of inquiry bearing upon this subject to which I wish to call your attention. It is a somewhat curious fact, that, while all modern writers admit the great antiquity of man, most of them maintain the very recent development of his intellect, and will hardly contemplate the possibility of men equal in mental capacity to ourselves, having existed in prehistoric times. This question is generally assumed to be settled, by such relics as have been preserved of the manufactures of the older races showing a lower and lower state of the arts; by the successive disappearance in early times of iron, bronze, and pottery; and by the ruder forms of the older flint implements. The weakness of this argument has been well shown by Mr. Albert Mott in his very original, but little known presidential address to the Literary and Philosophical Society of Liverpool in 1873. He maintains that "our most distant glimpses of the past are still of a world peopled as now with men both civilized and savage"—and, "that we have often entirely misread the past by supposing that the outward signs of civilization must always be the same, and must be such as are found among ourselves." In support of this view he adduces a variety of striking facts and ingenious arguments, a few of which I will briefly summarize.

On one of the most remote islands of the Pacific—Easter Island—2,000 miles from South America, 2,000 from the Marquesas, and more than 1,000 from the Gambier Islands, are found hundreds of gigantic stone images, now mostly in ruins, often thirty or forty feet high, while some seem to have been much larger, the crowns on their heads cut out of a red stone being sometimes ten feet in diameter, while even the head and neck of one is said to have been twenty feet high.* These once stood erect on extensive stone platforms, yet the island has only an area of about thirty square miles, or considerably less than Jersey. Now as one of the smallest images eight feet high weighs four tons, the largest must weigh over a hundred tons, if not much more; and the existence of such vast works implies a large population, abundance of food, and an established government. Yet how could these coexist in a mere speck of land wholly cut off from the rest of the world? Mr. Mott maintains that this necessarily implies the power of regular communication with larger islands or a continent, the arts of navigation, and a civilization much higher than now exists in any part of the Pacific. Very similar remains in other islands scattered widely over the Pacific add weight to this argument.

* Journ. of Roy. Geog. Soc., 1870, pp. 177, 178.

The next example is that of the ancient mounds and earthworks of the North American continent, the bearing of which is even more significant. Over the greater part of the extensive Mississippi valley four well-marked classes of these earthworks occur. Some are camps, or works of defence, situated on bluffs, promontories, or isolated hills; others are vast inclosures in the plains and lowlands, often of geometric forms, and having attached to them roadways or avenues often miles in length; a third are mounds corresponding to our tumuli, often seventy to ninety feet high, and some of them covering acres of ground; while a fourth group consist of representations of various animals modelled in relief on a gigantic scale, and occurring chiefly in an area somewhat to the north-west of the other classes, in the plains of Wisconsin.

The first class—the camps or fortified inclosures—resemble in general features the ancient camps of our own islands, but far surpass them in extent. Fort Hill, in Ohio, is surrounded by a wall and a ditch a mile and a half in length, part of the way cut through solid rock. Artificial reservoirs for water were made within it, while at one extremity, on a more elevated point, a keep is constructed with its separate defences and water-reservoirs. Another, called Clark's Work, in the Scioto valley, which seems to have been a fortified town, incloses an area of 127 acres, the embankments measuring three miles in length, and containing not less than three million cubic feet of earth. This area incloses numerous sacrificial mounds and symmetrical earth-works in which many interesting relics and works of art have been found.

The second class—the sacred inclosures—may be compared for extent and arrangement with Avebury or Carnak—but are in some respects even more remarkable. One of these, at Newark, Ohio, covers an area of several miles with its connected groups or circles, octagons, squares, ellipses, and avenues, on a grand scale, and formed by embankments from twenty to thirty feet in height. Other similar works occur in different parts of Ohio, and by accurate survey it is found not only that the circles are true, though some of them are one-third of a mile in diameter, but that other figures are truly square, each side being over 1,000 feet long, and, what is still more important, the dimensions of some of these geometrical figures in different parts of the country and seventy miles apart are identical. Now this proves the use, by the builders of these works, of some standard measures of length, while the accuracy of the squares, circles, and, in a less degree, of the octagonal figures—shows a considerable knowledge of rudimentary geometry, and some means of measuring angles. The difficulty of drawing such figures on a large scale is much

greater than any one would imagine who has not tried it, and the accuracy of these is far beyond what is necessary to satisfy the eye. We must therefore impute to these people the wish to make these figures as accurate as possible, and this wish is a greater proof of habitual skill and intellectual advancement than even the ability to draw such figures. If, then, we take into account this ability and this love of geometric truth, and further consider the dense population and civil organization implied by the construction of such extensive systematic works we must allow that these people had reached the earlier stages of a civilization of which no traces existed among the savage tribes who alone occupied the country when first visited by Europeans.

The animal mounds are of comparatively less importance for our present purpose, as they imply a somewhat lower grade of advancement; but the sepulchral and sacrificial mounds exist in vast numbers, and their partial exploration has yielded a quantity of articles and works of art, which throw some further light on the peculiarities of this mysterious people. Most of these mounds contain a large concave hearth or basis of burnt clay, of perfectly symmetrical form, on which are found deposited more or less abundant relics, all bearing traces of the action of fire. We are, therefore, only acquainted with such articles as are practically fire-proof. These consist of bone and copper implements and ornaments, discs, and tubes—pearl, shell and silver beads, more or less injured by the fire—ornaments cut in mica, ornamental pottery, and numbers of elaborate carvings in stone, mostly forming pipes for smoking. The metallic articles are all formed by hammering, but the execution is very good; plates of mica are found cut into scrolls and circles; the pottery, of which very few remains have been found, is far superior to that of any of the Indian tribes, since Dr. Wilson is of opinion that they must have been formed on a wheel, as they are often of uniform thickness throughout (sometimes not more than one-sixth of an inch) polished, and ornamented with scrolls and figures of birds and flowers in delicate relief. But the most instructive objects are the sculptured stone pipes, representing not only various easily recognizable animals, but also human heads, so well executed that they appear to be portraits. Among the animals, not only are such native forms as the panther, bear, otter, wolf, beaver, raccoon, heron, crow, turtle, frog, rattlesnake, and many others, well represented, but also the manatee, which perhaps then ascended the Mississippi as it now does the Amazon, and the toucan, which could hardly have been obtained nearer than Mexico. The sculptured heads are especially remarkable, because they present to us the features of an intellectual and civilized people. The nose in some is perfectly straight, and

neither prominent nor dilated, the mouth is small, and the lips thin, the chin and upper lip are short, contrasting with the ponderous jaw of the modern Indian, while the cheek-bones present no marked prominence. Other examples have the nose somewhat projecting at the apex in a manner quite unlike the features of any American indigenes, and, although there are some which show a much coarser face, it is very difficult to see in any of them that close resemblance to the Indian type which these sculptures have been said to exhibit. The few authentic crania from the mounds present corresponding features, being far more symmetrical and better developed in the frontal region than those of any American tribes, although somewhat resembling them in the occipital outline;* while one was described by its discoverer (Mr. W. Marshall Anderson) as "a beautiful skull worthy of a Greek."

The antiquity of this remarkable race may perhaps not be very great, as compared with the prehistoric man of Europe, although the opinions of some writers on the subject seem affected by that "parsimony of time" on which the late Sir Charles Lyell so often dilated. The mounds are all overgrown with dense forest, and one of the large trees was estimated to be eight hundred years old, while other observers consider the forest growth to indicate an age of at least 1,000 years. But it is well known that it requires several generations of trees to pass away before the growth on a deserted clearing comes to correspond with that of the surrounding virgin forest, while this forest, once established, may go on growing for an unknown number of thousands of years. The 800 or 1,000 years' estimate from the growth of existing vegetation is a minimum which has no bearing whatever on the actual age of these mounds, and we might almost as well attempt to determine the time of the glacial epoch from the age of the pines or oaks which now grow on the moraines.

The important thing for us, however, is that when North America was first settled by Europeans, the Indian tribes inhabiting it had no knowledge or tradition of any preceding race of higher civilization than themselves. Yet we find that such a race existed; that they must have been populous and have lived under some established government; while there are signs that they practised agriculture largely, as indeed they must have done to have supported a population capable of executing such gigantic works in such vast profusion—for it is stated that the mounds and earthworks of various kinds in the State of Ohio alone amount to between eleven and twelve thousand. In their habits, customs, religion, and arts, they differed strikingly from all the Indian tribes; while their love

* Wilson's "Prehistoric Man," 3d ed., vol. ii, pp. 123–130.

of art and of geometric forms, and their capacity for executing the latter upon so gigantic a scale, render it probable that they were a really civilized people, although the form their civilization took may have been very different from that of later people subject to very different influences, and the inheritors of a longer series of ancestral civilizations. We have here, at all events, a striking example of the transition, over an extensive country, from comparative civilization to comparative barbarism, the former having left no tradition, and hardly any trace of influence on the latter.

As Mr. Mott well remarks:—Nothing can be more striking than the fact that Easter Island and North America both give the same testimony as to the origin of the savage life found in them, although in all circumstances and surroundings the two cases are so different. If no stone monuments had been constructed in Easter Island, or mounds, containing a few relics saved from fire, in the United States, we might never have suspected the existence of these ancient peoples. He argues, therefore, that it is very easy for the records of an ancient nation's life entirely to perish, or to be hidden from observation. Even the arts of Nineveh and Babylon were unknown only a generation ago, and we have only just discovered the facts about the mound-builders of North America.

But other parts of the American continent exhibit parallel phenomena. Recent investigations show that in Mexico, Central America, and Peru, the existing race of Indians has been preceded by a distinct and more civilized race. This is proved by the sculptures of the ruined cities of Central America, by the more ancient terra-cottas and paintings of Mexico, and by the oldest protrait-pottery of Peru. All alike show markedly non-Indian features, while they often closely resemble modern European types. Ancient crania, too, have been found in all these countries, presenting very different characters from those of any of the modern indigenous races of America.*

There is one other striking example of a higher being succeeded by a lower degree of knowledge, which is in danger of being forgotten because it has been made the foundation of theories which seem wild and fantastic, and are probably in great part erroneous. I allude to the Great Pyramid of Egypt, whose form, dimensions, structure, and uses have recently been the subject of elaborate works by Professor Piazzi Smyth. Now, the admitted fact about this pyramid are so interesting and so apposite to the subject we are considering, that I beg to recall them to your attention. Most of you are aware that this pyramid has been carefully explored and measured by successive Egyptologists, and that the dimensions have lately become

* Wilson's "Prehistoric Man," 3d ed., vol. ii, pp. 125, 144.

capable of more accurate determination owing to the discovery of some of the original casing-stones and the clearing away of the earth from the corners of the foundation, showing the sockets in which the corner-stones fitted. Professor Smyth devoted many months of work with the best instruments in order to fix the dimensions and angles of all accessible parts of the structure; and he has carefully determined these by a comparison of his own and all previous measures, the best of which agree pretty closely with each other. The results arrived at are—

1. That the pyramid is truly square, the sides being equal and the angles right angles.
2. That the four sockets on which the four first stones of the corners rested are truly on the same level.
3. That the direction of the sides are accurately to the four cardinal points.
4. That the vertical height of the pyramid bears the same proportion to its circumference at the base, as the radius of a circle does to its circumference.

Now all these measures, angles, and levels are accurate, not as an ordinary surveyor or builder could make them, but to such a degree as requires the very best modern instruments and all the refinements of geodetical science to discover any error at all. In addition to this we have the wonderful perfection of the workmanship in the interior of the pyramid, the passages and chambers being lined with huge blocks of stones fitted with the utmost accuracy, while every part of the building exhibits the highest structural science.

In all these respects this largest pyramid surpasses every other in Egypt. Yet it is universally admitted to be the oldest, and also the oldest historical building in the world.

Now these admitted facts about the Great Pyramid are surely remarkable, and worthy of the deepest consideration. They are facts which, in the pregnant words of the late Sir John Herschel, "according to received theories ought not to happen," and which, he tells us, should therefore be kept ever present to our minds, since "they belong to the class of facts which serve as the clue to new discoveries." According to modern theories, the higher civilization is ever a growth and an outcome from a preceding lower state; and it is inferred that this progress is visible to us throughout all history and in all the material records of human intellect. But here we have a building which marks the very dawn of history—which is the oldest authentic monument of man's genius and skill, and which, instead of being far inferior, is very much superior to all which followed it. Great men are the products of their age and country, and the designer and constructors of this wonderful monument

could never have arisen among an unintellectual and half-barbarous people. So perfect a work implies many preceding less perfect works which have disappeared. It marks the culminating point of an ancient civilization, of the early stages of which we have no record whatever.

The three cases to which I have now adverted (and there are many others) seem to require for their satisfactory interpretation a somewhat different view of human progress from that which is now generally accepted. Taken in connection with the great intellectual power of the ancient Greeks—which Mr. Galton believes to have been far above that of the average of any modern nation—and the elevation, at once intellectual and moral, displayed in the writings of Confucius, Zoroaster, and the Vedas, they point to the conclusion, that, while in material progress there has been a tolerably steady advance, man's intellectual and moral development reached almost its highest level in a very remote past. The lower, the more animal, but often the more energetic types, have however always been far the more numerous; hence such established societies as have here and there arisen under the guidance of higher minds, have always been liable to be swept away by the incursions of barbarians. Thus in almost every part of the globe there may have been a long succession of partial civilization, each in turn succeeded by a period of barbarism; and this view seems supported by the occurrence of degraded types of skull along with such "as might have belonged to a philosopher"—at a time when the mammoth and the reindeer inhabited southern France.

Nor need we fear that there is not time enough for the rise and decay of so many successive civilizations as this view would imply; for the opinion is now gaining ground among geologists that paleolithic man was really preglacial, and that the great gap—marked alike by a change of physical conditions, and of animal life—which in Europe always separates him from his neolithic successor, was caused by the coming on and passing away of the great ice age.

If the views now advanced are correct, many, perhaps most, of our existing savages, are the successors of higher races; and their arts, often showing a wonderful similarity in distant continents, may have been derived from a common source among more civilized peoples.

I must now conclude this very imperfect sketch of a few of the offshoots from the great tree of Biological study. It will, perhaps, be thought by some that my remarks have tended to the depreciation of our science, by hinting at imperfections in our knowledge and errors in our theories, where more enthusiastic students see nothing but established truths. But I trust that I may have conveyed to many of my hearers a different

impression. I have endeavored to show that even in what are usually considered the more trivial and superficial characters presented by natural objects, a whole field of new inquiry is opened up to us by the study of distribution and local conditions. And as regards man, I have endeavored to fix your attention on a class of facts which indicate that the course of his development has been far less direct and simple than has hitherto been supposed; and that, instead of resembling a single tide with its advancing and receding ripples, it must rather be compared to the progress from neap to spring tides, both the rise and the depression being comparatively greater as the waters of true civilization slowly advance towards the highest level they can reach.

And if we are thus led to believe that our present knowledge of nature is somewhat less complete than we have been accustomed to consider it, this is only what we might expect; for however great may have been the intellectual triumphs of the nineteenth century, we can hardly think so highly of its achievements as to imagine that, in somewhat less than twenty years, we have passed from complete ignorance to almost perfect knowledge on two such vast and complex subjects as the origin of species and the antiquity of man.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Pyrogenic Hydrocarbons in Coal Gas.*—Berthelot has made a study of the hydro-carbons in coal gas, which tends to confirm his theory of the formation of these bodies by the action of acetylene and hydrogen at high temperatures. In the Paris gas for example, he finds that he can detect benzene by exposing two or three cubic centimeters to a drop of fuming nitric acid. On diluting with water, the characteristic odor of nitrobenzene appears. Fifty liters of gas passed through 8 or 10 c.c. of the acid, gives enough nitrobenzene when diluted, to weigh; from which it appears that the gas contains two or three volumes of benzene vapor in the hundred. More accurate determination gives 3 to 3·5 volumes. Next to methane, therefore, benzene is the principal hydrocarbon in this gas and is the illuminant, *par excellence*. Ethylene and acetylene, though present, exist in minute proportion, only two to three thousandths. Propylene, butylene, and allylene are found, in amount about two-tenths per cent. They were detected by passing the gas first through sulphuric acid diluted with its own volume of water, and then through a column of pumice stone wet with concentrated sulphuric acid. A tarry substance collected in the first vessel, which yielded

no products volatile below 360°–400°, and was probably composed of polymers of some easily alterable hydrocarbon. The acid itself, fractionated, gave acetone, 0·25 gram per 100 cubic meters, coming from the hydration of allylene. The sulphuric acid collected beneath the pumice column gave two layers. The lower one consisted of the acid, more or less altered. Diluted with water, a tarry substance of high boiling point separated, probably polymerized hydrocarbons. The acid liquid gave isopropyl alcohol on distillation, thus proving the existence of propylene. The upper layer of liquid, in amount about 25 grams to 100 cubic meters of gas, consisted of hydrocarbons, and gave on fractioning, benzene (with a little toluene) 2 per cent, mesitylene (C_9H_{12}) 5 per cent, cymene ($C_{10}H_{14}$) 20 per cent, tricrotonylene ($C_{12}H_{18}$) 30 per cent, colophene ($C_{15}H_{24}$) 32 per cent, residue fixed at 320° 5 per cent, intermediate products and loss 6 per cent =100. In one million volumes of this gas, consequently, there are by this analysis:

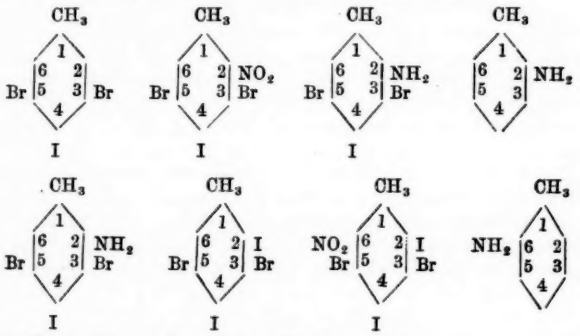
Benzene (C_6H_6) in vapor	30000 to 35000	
Acetylene (C_2H_2)	1000 (about)	
Ethylene (C_2H_4)	1000 to 2000	
Propylene (C_3H_6)	2·5	
Allylene (C_3H_4)	8	
Butylene (C_4H_8) and analogues	traces	
Crotonylene (C_4H_6)	31	181
Terene (C_5H_8)	12	
Hydrocarb's transfor'd into fixed polymers, est'd	83	
Diacetylene and analogous hydrocarbons, est'd	15	

The author regards these products as derived according to the reactions upon which he founds his theory, from the four fundamental hydrocarbons acetylene C_2H_2, ethylene C_2H_4, dimethyl C_2H_6, and methane CH_4. These, together with hydrogen forming a system in equilibrium, such that, at a red heat they are all formed from any one of them as the starting point. Thus from methane comes directly ethylene (C_2H_4) or $(CH_2)_2$, propylene $(CH_2)_3$ and the series of polymers $(CH_2)_n$. Acetylene produces benzene C_6H_6 or $(C_2H_2)_3$, and the series of polymers $(C_2H_2)_n$. Moreover, from the union of two of these fundamental hydrocarbons, more complex bodies come: acetylene and benzene giving styrolene C_8H_8; acetylene and styrolene, naphthalene $C_{10}H_8$; acetylene and naphthalene, acenaphthene $C_{12}H_{10}$; and styrolene and benzene, anthracene $C_{14}H_{10}$. So acetylene combines at a dull red heat with ethylene to form ethylacetylene C_4H_6, and with propylene to yield propylacetylene C_5H_8, the former identical with crotonylene, the latter with terene. The uselessness of the present eudiometric method of determining the illuminants in gas analysis is obvious from these results.—*Bull. Soc. Ch.*, II, xxvi, 104, Aug., 1876. G. F. B.

2. *Occurrence of Benzene in Rosin Oil.*—Watson Smith has examined the light oils obtained as a bye-product in the refining

of rosin by distillation in a current of super-heated steam. In 1867, he examined a sample which began to boil at 50° and which distilled almost completely below 100°. On fractioning, a liquid was obtained boiling between 80° and 85° which had the properties of benzene, though contaminated with turpentine-products. Meantime changes had been made in the works, a much higher temperature being employed. A sample from the first run of the rectifying still, examined in 1875, began to boil at 109°, and to condense at 116°. When fractionated, toluene was the substance of lowest boiling point obtained. Hence the temperature of distillation determines the products.—*J. Chem. Soc.*, II, xxx, 29, July, 1876. G. F. B.

3. *On the Constitution of the Benzene Derivatives.*—Wroblewsky described in 1875 two metabromtoluenes having identical properties, in one of which the methyl and the bromine atoms occupied the 1 : 3 position according to Kekulé's theory, in the other the 1 : 5 position. He now describes two ortho-toluidines having the methyl and amidogen groups respectively in the positions 1 : 2 and 1 : 6. Starting with dibromparatoluidine, in which the two bromine atoms occupy the positions 3 and 5, the methyl and amidogen groups 1 and 4, the author replaced the amidogen by iodine, thus forming $C_7H_5Br_2I$, dibromparaiodtoluene. By the action of fuming nitric acid a nitroderivative $C_7H_4Br_2INO_2$ was obtained, in which the nitryl must occupy the position 2 or the position 6. On reduction with tin and hydrochloric acid this gave $C_7H_4Br_2INH_2$, and this by the action of sodium amalgam gave orthotoluidine, $C_7H_7NH_2$. For the second substance, the metadibrom-paraiod-ortho-toluidine was converted by the method of Griess into $C_7H_4Br_2I_2$ dibrom-diiod-toluene. This gave a nitroderivative $C_7H_3Br_2I_2NO_2$, in which the nitryl must occupy the position 6 if the former had 2; or the reverse. By reducing the amido-product obtained from this with sodium amalgam, a second ortho-toluidine was obtained, identical in properties with the former, thus adding a new confirmation to Kekulé's theory. The progress of the replacements is thus represented:



—*Ber. Berl. Chem. Ges.*, ix, 1055, July, 1876. G. F. B.

4. *On the Action of Malt-extract on Starch.*—O'SULLIVAN has examined more fully the conditions under which malt-extract acts on starch. He formulates his conclusions as follows: (1) Maltose and dextrin are the only products of the action of malt-extract on starch. (2) Cold malt-extract does not act on ungelatinized starch. (3) Malt-extract begins to dissolve starch at the temperature of gelatinization or a few degrees lower. (4) Malt-extract dissolves gelatinized starch in the cold, (10° to 20°) almost completely if the gelatinization be perfect. (5) When starch is dissolved by malt-extract at any temperature below 63°, if the solution be immediately (5 or 10 minutes) cooled and filtered, the product invariably contains maltose and dextrin in proportions agreeing closely with 67·85 per cent of the former, and 32·15 per cent of the latter. (6) If the temperature of the action be between 64° and 68°–70°, the maltose present is 34·54 per cent and the dextrin 65·46 per cent. (7) If the temperature be between 68° and 70° and the point at which the activity of the transforming agent is destroyed, the maltose and dextrin are in the proportion of 17·4 to 82·6 per cent. The decomposition of starch into maltose and dextrin is molecular and takes place according to three equations, corresponding to the conditions in (5), (6) and (7) above. Dextrin is converted into maltose by a slow and gradual process of hydration. —*J. Chem. Soc.*, xxx, 125, Aug., 1876. G. F. B.

5. *Detection of Carbamic acid in Animal fluids.*—HOFMEISTER has examined the evidence upon which Drechsel based his assertion that carbamic acid is produced wherever nitrogenous substances are oxidized in alkaline solutions, and hence exists in the blood. He finds that the reactions upon which Drechsel based his opinion are untrustworthy, inasmuch as the production of a precipitate on boiling, after having filtered off the precipitate produced by calcium nitrate, cannot be taken as proof of the existence of carbamic acid. - *J. pr. Ch.*, II, xiv, 173, Aug., 1876. G. F. B.

6. *Friction of Gases.*—M. WIEDEMANN has measured the changes in the coefficients of friction of gases with changes in the temperature by a new form of apparatus. In a recent paper on the specific heat of gases he claims that a gas undergoes a sort of dissociation upon a change of temperature and that the diameter of the molecules ought not to vary with the temperature according to the same law as if the gas was not decomposed. Moreover, according to the new theory of gases, the coefficient of friction of gases gives a relative measure of the diameter of its molecules. The gas is contained in two glass bulbs 7·3 cms. in diameter and and 4·5 cms. high. One is placed above the other and they are connected by a glass tube ·8 cms. in diameter and 15 cms. long. They are enclosed in a case with glass sides which may be filled with water. Each bulb contains in the prolongation of the tube connecting them, a small orifice. To the lower one is connected a three-way cock with two horizontal tubes. One of these tubes is connected with a reservoir of mercury of variable height, by which mercury may be admitted into the bulbs. The other tube

allows the mercury to pass out. The upper bulb is connected with a water manometer and drying tube and with a capillary tube. The latter is attached to a larger tube 1·8 cms. in diameter and 4·8 cms. long, filled with copper turnings by which the gas is brought to any required temperature before entering the capillary. Both these tubes can be immersed in a vessel of cold water or in a tube with a double wall in which circulates the vapor of aniline or water.

To make the experiment the end of the capillary tube is closed until the top of the mercurial column stands near a point marked on the vertical tube. The capillary is then opened and at the instant the mercury passes the mark, a stop-watch is started and the pressure read by the manometer. This pressure is maintained constant during the whole experiment by slowly raising the mercury reservoir. At the end of 5 to 15 minutes the mercury supply is cut off and that in the bulbs is drawn off until it stands at the level of the mark. The weight of mercury gives the volume of gas transpired. The following table gives the result of the measurement of six gases at temperatures, 8°, 100° and 184·5°. The first column gives the name of the gas, the next three the coefficients of friction at 100° and 184·5° taking the coefficient of air as 100. The last two columns give the friction compared with that of air at 8°.

	0°	100°	184·5°	100°	184·5°
Air	100	100	100	123·1	141·1
Carbonic oxide	96·87	----	96·42	----	136·0
Carbon acid	80·5	85·63	87·50	104·8	123·4
Protoxide nitrogen	80·5	85·82	87·94	105·6	124·1
Ethylene	56·24	60·02	61·93	73·89	87·38
Hydrogen	51·51	51·81			

It is commonly assumed that the friction is proportional to some power n of the absolute temperature. But a computation with the above values shows that no value of n will satisfy all the observations.—*Bib. Univ.*, ccxxiii, 277. E. C. P.

7. *Effect of Sound on the Radiometer.*—M. Jeannel has observed that certain sonorous vibrations cause rotatory movements in the radiometer. In half obscurity, three radiometers were placed on the interior tablet of a chamber organ. The bass notes, those of the three first octaves, produced rotation, the most bass acting most, but *fa* and *fa* sharp of the lower octave (especially with the bourdon stop) produced more rapid rotation than *ut*, *re* and *mi*, though these are more grave. Radiometers do not all act in the same manner, as to rapidity and direction of their rotation. Thus to the low *fa* or *fa* sharp, radiometer A, the less sensitive to light made about one turn per second. The black faces first (i. e. a direction opposite to that produced by light), whilst radiometers B and C, which were more sensitive to light, turned more slowly in the direction of the movement produced by light. M. Jeannel explains these effects by circular or angular vibrations of the supporting needle transmitted from the tablet of the organ. By applying the finger to the top of the radiometer,

one may prevent the vibration and also the rotation. The board of a piano produces similar effects but in a less degree. If the experiments be made where the diffuse light is nearly sufficient to drive the radiometer, grave sounds, even the weakest, cause rotation in the ordinary direction (bright surfaces first); the rumble of a vehicle will suffice. Here the light is at first insufficient to overcome the friction, but when the vibrations intervene, friction is lessened during certain intervals, and the apparatus is thus rendered more sensitive to light.—*Nature*, xiv, 419. E. C. P.

8. *Fusion of Soft Bodies.*—M. PFAUNDLER has presented to the Imperial Academy of Sciences at Vienna, papers on the nature of the soft or half liquid state of aggregation, and on regelation and recrystallization. After dividing the bodies in question into mixtures of small solid parts with true liquids, soft bodies proper, containing no dissimilar parts, and mixtures of the two classes, he gives a hypothesis on the process of melting and the soft state. The common ideal melting process, when the temperature remains the same from the beginning to the end is not according to fact. The mean temperature of the body beginning to melt is about $t+t'$ lower than that of the already melted mass, if $\pm t$ and $\pm t'$ denote the amounts of divergence of temperature of the separate molecules in the solid and liquid condition. Hence the true melting point is different from that at the beginning, and the end of the melting process. M. Pfaundler extends his hypothesis to soft bodies of a compound nature, and to regelation and recrystallization.—*Nature*, xiv, 383. E. C. P.

9. *Friction of Gases.*—Professor KUNDT, in a popular lecture on "the Modern Theory of Gases," delivered last March in Berlin, made an ingenious application of the principle of the radiometer. Having first shown the friction of air of ordinary density by means of a rotating disk, the lecturer then took an exhausted vessel containing two disks, each about three centimeters in diameter supported very close to one another; the lower one had the usual vanes of the radiometer while the upper one had only a couple of small projecting wires sufficient to show its motion. A magnesium lamp served for projecting the instrument on a screen, and to rotate the lower disk; through the minute amount of air remaining in the vessel the upper disk was put promptly into rotation.

Probably the same principle will find other applications when rotation in a vacuum is to be produced. C. K. W.

10. *On the Electro-magnetic action of Electric Convection;* by Dr. HELMHOLTZ. A report on some experiments carried out by Mr. Henry A. Rowland, of the Johns Hopkins University, in Baltimore.—I understand by electric convection the conveyance of electricity by the motion of its ponderable bearers. In my last memoir on the theory of electro-dynamics,* I proposed some experiments (which were then carried out by Herr N. Schiller) in which the question came into consideration whether electric convection is dynamically equivalent to the flow of electricity in a

* *Monatsbericht* of the Berlin Academy of Sciences, June 17, 1875, p. 405.

conductor, as W. Weber's theory assumes. Those experiments might possibly have been decisive against the existence of such an action. They were not so; but, on the other hand, through this negative result the existence of the action in question remained unproved. Mr. Rowland has now carried out a series of direct experiments, in the physical laboratory of the University here, which give positive proof that the motion of electrified ponderable substances is also electro-magnetically operative. I here remark that he had already conceived and fully considered the plan of his experiments when he came to Berlin, without any previous coöperation on my part.

The moved bearer of the electricity was a disk of ebonite 21·1 centims. in diameter and ½ centim. thick. It could revolve with great velocity (up to 61 times in a second) about a vertical axis fixed in its center. The ebonite disk was gilt on both sides; but the gilding was insulated from the axis. Near it, above and below, were placed glass disks of 38·9 centims. diameter, pierced through the middle to admit the axis of the ebonite disk. The glass disks were likewise gilt, in an annular band of 24 centims. external, 8·9 internal diameter; the gilt side was mostly turned toward the ebonite disk. The gilt surfaces of the glass disks were, as a rule, connected to earth; while the ebonite between them, through a point directed toward it at a distance of ⅓ millim. from its margin, was in electrical communication with the coatings of a large insulated Leyden battery which served as a reservoir for the electricity. A commutator of a peculiar construction, inserted between them, permitted now the one, now the other coating to be connected either with the ebonite disk or with the earth. In the construction of these parts, iron was avoided.

Close above the upper glass disk an extremely sensitive astatic needle was suspended to an arm fixed in the wall, and completely enclosed in a brass case connected to earth. The two needles were 1·5 centim. long, but at a considerable distance (17·98 centims.) from each other. Their deflections were read off with a mirror and a telescope. The opening in front of the mirror was protected from external electrical influences by a metallic hollow cone. Indeed the electrical charge of the battery and the reversal of the electrification of the ebonite disk gave no perceptible trace of action on the needle so long as the ebonite was stationary.

On the other hand, on swift rotation, even without electrifying, the action of rotation-magnetism was shown, mostly arising from the brass axis of the rotating disk, and considerably diminished by reducing it to 0·9 centim. thickness. The action of the electrification of the disk could be separated from that of the rotation-magnetism by letting in alternately positive and negative electricity (by means of the commutator above-mentioned) while the velocity of the rotation was maintained unaltered. The displacement of the needle from the position of equilibrium amounted to from 5 to 7½, its arc of oscillation on changing the electrification, therefore, to from 10 to 15 scale-divisions. This result ensued in hundreds of

observations (which were made with gradually and continually more and more improved apparatus in the course of several weeks), and always in the same direction. The direction of the deflection of the needle, the length of which was normal to the radius of the disk, was such as would have been produced by a positive electric current flowing with the rotation of the positively charged disk, or against the rotation of the disk charged negatively.

There was no alteration in the action when the gilding of the ebonite plate was removed in a series of radial lines, so that annular electric currents could no longer take place. A thin plate of glass was also inserted instead of the gilt ebonite, and, like the disk of a Holtz machine, could be electrified through points; while close beneath it there was a gilt resting plate connected to earth, in order to fix as much electricity as possible. The direction of the deflections was the same as in the previous experiments: but they were smaller, as the conditions were not so favorable for strong electrification.

In order to compare the electricity carried forward by convection with that which passes in conductors, experiments were instituted in the following manner.

The ebonite disk was gilded afresh, and the gold coating divided, by a series of fine circular lines, into rings insulated from one another. The innermost ring was connected with the axis; the rest could not at any rate become considerably charged without discharging themselves by very short sparks from one to the other. Two electrified plates, each having the form of a sector of a circle, but which did not reach to the axis, were placed, opposite to one another, above and below the rotating plate. Under these circumstances the electricity of the gold rings must have been accumulated by electrostatic induction in the sector covered by the last-mentioned plates, and carried forward convectively. When this electricity was positive, it became free at the fore margin of the induced sector (in the direction of the rotation), while at the hind margin of the same, continually new positive electricity being attached, relatively negative electricity became free.

The positive electricity must, under these conditions, have overflowed from the fore to the hind margin of the sector, for which there were in each ring two paths open, between which it must have divided itself in the inverse ratio of their resistances. If the inducing sector comprises $\frac{1}{n}$ of the circumference, the resistance of the path in the sector is to that of the path outside of it as $1 : n-1$; and therefore $\frac{n-1}{n}$ of the current returns through the sector, and $\frac{1}{n}$ outside of it. In the sector a quantity corresponding to the sum of the two currents is carried forward against the current by convection. If, then, a convective motion of electricity

acts like a conducted motion, the total motion in the sector is $1-\frac{n-1}{n}=\frac{1}{n}$. But if the action of convective had been greater or less than that of conducted motion, the excess, in one or the other direction, must have been shown on the sector.

The experiments showed that, when the sector was small ($\frac{1}{8}$ of the circumference), the small difference between the convection 1 and the conduction $\frac{7}{8}$ in general could not (or at least not with certainty) be detected, that therefore, with approximate equality of convection and conduction, the electro-dynamic effect of the one sensibly neutralized that of the other.

When, however, the sector took in half of the circumference, the here assumed current could be observed even in the free portion of the disk, though the amount was too small for safe measurement.

The observed electro-dynamic action being so little in the foregoing experiments, in which the disk was electrified and covered in its whole extent by the induced plates, theoretical calculation of the amount of the action from the known absolute values of the electro-dynamic constants promised only approximately accordant values. Nevertheless it was carried out by Mr. Rowland.

The proportion in which the action of the earth's magnetism upon the pair of astatic needles was diminished was ascertained by finding the oscillation-period, first with the needles equally directed, and then arranged astatically.

The value of the function of the electric potential in the Leyden battery, and on the rotating disk, was determined according to the law of spark-length given by Sir William Thomson, which in this case appeared sufficiently accurate. Before and after each experiment, a smaller jar was charged from the battery of nine large ones containing the store of electricity, and on it the length of the spark was determined.

The velocity of the rotation was regulated by the position of the balls of a centrifugal governor, which was applied to one of the more slowly rotating axes. The calculation from the magnitude of the rollers agreed well with the determination by the tone of a siren-disk, which was for some time attached to the most rapidly revolving axis.

In the calculation of the distribution of the electricity on the disk, and its electro-magnetic directing-force, the surplus charge present at the margin of the disk was reckoned according to the value which holds for infinitely thin disks, and considered as an infinitely thin thread concentrated at the margin—a proceeding which was in both ways only approximately correct, but, in presence of the minuteness of this portion, was sufficient.

The influence exerted upon the upper needle was about $\frac{1}{50}$ of that upon the lower. The horizontal force of the earth's magnetism was put equal to 0·182 (using the centimeter, gram, and second as units): the electro-dynamic constant was put by Mr.

Rowland, after Maxwell's determinations, equal to 28,800 millions. W. Weber's value would be 31,074 millions. I give below under M. the results calculated with the former value, under W. those calculated with the latter.

The following is the result of the calculation of only three series of experiments conducted under favorable circumstances:—

(1) Ten experiments with alternately opposite rotation. In each, three readings, of which the middle one was made with the electrification of the disk opposite to that of the first and third.

Mean difference of the position of equilibrium, in scale-divisions	6·735
Spark-length	0·2845
Electro-dynamic force acting on the astatic pair—observed	0·00000327
" " " " calculated, M.	0·00000337
" " " " " W.	0·00000311

(2) Four experiments, the same.

Difference of the position	7·50
Spark-length	0·2955
Electro-dynamic force—observed	0·00000317
" " calculated, M.	0·00000349
" " " W.	0·00000322

(3) Five experiments, the same.

Difference of the position	7·60
Spark-length	0·2926
Electro-dynamic force—observed	0·00000339
" " calculated, M.	0·00000355
" " " W.	0·00000328

The accordance may be looked upon as satisfactory in the measurement of a force which amounts to only $\frac{1}{50000}$ of the force of the earth's magnetism, since in two of the series the observed values fall between those corresponding to the different measured values of Weber's constant.

As regards the signification of these experiments for the theory of electro-dynamics, they correspond to the hypotheses of the theory of W. Weber; but they can also be referred to Maxwell's, or to the potential-theory which takes account of the di-electric polarization of the insulators. The volume-elements of the stratum of air situated between the resting and the moved plates suffer continual displacements in the direction of a rotation round radially directed rotation-axes. The existing di-electric polarization of these elements will therefore in each material element continually change, while retaining in space the same direction normal to the surface of the electrified disks. The arising and disappearing components of this polarization would constitute the current which is indicated by the astatic pair of needles.—*Monatsbericht der kön. preuss. Akademie der Wissenschaften zu Berlin*, 1876, pp. 211–216; *Phil. Mag.*, Sept. 1876.

II. Geology and Mineralogy.

1. *Note on specimen of Metadiabase from Connecticut Lake, collected and sliced by G. W. Hawes.*—The fragments, apparently organic, in this slice agree with that figured by Mr. Hawes in the August number of the American Journal of Science, Plate v, fig. 5. They consist of a transparent brownish substance, traversed by parallel bars or ribs of greater transparency. In places the bars are interrupted abruptly, and crossed by similar bars nearly at right angles to the others. Between the longitudinal bars are irregular transverse and oblique lines; but these are not properly structural, and appear to be cracks, some of which are open and transparent, but the greater part closed and of a black color. From the irregular shape of the fragments, and the manner in which the ends project in shreds, it is to be inferred that the substance, if organic, was not hard or stony like a coral, but tough and soft, and in an advanced stage of decay. Fragments of the horny investment of Hydroids or Bryozoans might present such appearances, and in this case the planes where the bars are interrupted may represent the mouths of cells. Again, the chitinous crust of some Entomostracans, as for example, species of *Dithyrocaris*, shows bands not very dissimilar from those in the fragments, which, however, present no trace of the cellular structure usual in such crusts. Again, the Devonian plants of the genus *Dictyophyton* show a rectangular areolation which, though much coarser, reminds one of these specimens. Lastly, Gumbel has figured from the Laurentian of Bavaria certain films with rectangular meshes, much finer than those of Mr. Hawes' specimens, but not unlike them in appearance, and which he regards as organic. On the whole, though these objects are unlike any purely mineral substance with which I am acquainted, and are probably fragments of some organic body, I do not think it possible at present to indicate with any certainty their probable affinities.

Sept. 7, 1876. J. W. DAWSON.

2. *On Streams of Water beneath Glaciers.* — Mr. Charles Knight, in the Philosophical Magazine for June, states that, according to Professor Wm. Thomson's experiments, the freezing point of water is lowered 0°·23 F. for each additional atmospheric pressure; and that, hence, if a glacier have a thickness of 3,000 feet, the pressure would be about 80 atmospheres, and under this pressure the temperature at the base should not exceed 13° F. to retain the solid form. The statement needs a correction, since Professor Wm. Thomson's experiment made the lowering of the freezing point of water 0°·23 F. for *sixteen* atmospheres of pressure. This would give for 80 atmospheres, supposing the increase by arithmetical ratio, only 1°·15 F.

3. *Quinto Apendice al reino mineral de Chile i de las Republicas vecinas publicado en la segunda edicion de la Mineralojia de don* Ignacio Domeyko. 79 pp. 8vo. Santiago, 1876.—The fifth

Appendix by Domeyko to his Mineralogy of Chili follows the fourth after an interval of two years. It contains much valuable and interesting matter, including the description of the following new minerals:

Daubreite.—Amorphous. In structure earthy and compact, in parts fibrous. H.=2·5. G.=6·4–6·5. Color yellowish to grayish-white. Opaque. An analysis gave Bi_2O_3 89·60, Cl 7·50, H_2O 3·84(?), Fe_2O_3 0·72, which corresponds to the formula $(Bi_2O_3)_4$, Bi_2Cl_3. This formula places the mineral in the series with the two artificial compounds $(Bi_2O_3)_2$, Bi_2Cl_6, and $(Bi_2O_3)_6$, Bi_2Cl_3. It is easily soluble in hydrochloric acid, without residue. Locality, Constancia Mine, Cerro de Tazna, Bolivia.

Krönkite.—In irregular crystalline masses with coarse fibrous structure; probably triclinic. Cleavage distinct parallel to an edge of the prism. Color azure-blue, changing somewhat on exposure to the air. Luster vitreous; translucent. Composition $CuSO_4+Na_2SO_4+2H_2O$, which requires $CuSO_4$ 47·23, Na_2SO_4 42·09, H_2O 10·68=100. Perfectly soluble in water. Found in the copper mines near Calama, on the road from Cabija to Potosi, Bolivia.

Phillipite.—Forms small irregular masses and bands in the same argillacous ochre in which the copper pyrites occurs, by the decomposition of which it has been formed. Structure fibrous, sometimes compact; never prismatic like Krönkite. Color azure-blue. Luster vitreous; translucent. Soluble in water, but unaffected by exposure to the air. Composition $CuSO_4+FeS_3O_{12}+n\,aq.$ Analysis gave SO_3 28·96, FeO_3 9·80, CuO 14·39, MgO 0·85, H_2O 43·72 AlO_3 *tr*=100·00. Found at the copper mines in the Cordilleras of Condes, Province of Santiago, Chili.

Huantajayite.—Isometric. Crystallizes in cubes like the chlorides of sodium and silver. H.=2. Transparent. Color white, not altered by exposure to the air. Fragile, easily reduced to a powder, not sectile like cerargyrite. Composition 20NaCl+AgCl; an analysis gave NaCl 89, AgCl 11=100. B.B. decrepitates and fuses easily, losing its transparency; with soda yields metallic silver. Found at the mine of San Simon, in the Cerro de Huanjayita.

The descriptions of the following new minerals in the Appendix are given by Sr. Raimondi.

Cuprocalcite.—Occurs in small masses, and in bands intimately mixed with a ferruginous carbonate of calcium. H.=3; G.=3·90. Color bright vermillion-red. The analysis gave Cu_2O 50·45, CaO 20·16, CO_2 24·00, H_2O 3·20, FeO_3 0·60, AlO_3 0·20, MgO 0·97, SiO_2 0·30=99·88. This leads to the formula $(Cu_2O)_2CO_2+2CaCO_3+H_2O$, which requires Cu_2O 52·2, CaO 20·4, CO_2 24·1, H_2O 3·3=100. Soluble in hydrochloric acid with effervescence. The solution, formed with exclusion of the air, has a strong deoxydizing power, precipitating metallic gold from solutions of gold salts. Found at the mines of Canza, near the city of Ica, in Peru.

Werthemanite.—Occurs in powder, or in masses easily reduced to powder. Color white. Gives an argillaceous odor, and adheres

to the tongue. G.=2·80. Soluble only in sulphuric acid. An analysis gave SO_3 34·50, AlO_3 45·00, FeO_3 1·25, H_2O 19·25=100. This affords the formula $AlSO_6+3H_2O$; or like aluminite except in the smaller amount of water. Found near the city of Chachapoyas.

Malinowskite.—A variety of tetrahedrite. An analysis of the mineral from the district of Rocuay gave S 24·27, Sb 24·74, As 0·56, Pb 13·08, Cu 14·37, Ag 11·92, Fe 9·12, Zn 1·92=100. Other analyses of the same mineral from the mine of Carpa agree closely with this. It occurs massive, and has a gray color and metallic luster. E. S. D.

III. Botany.

1. *Flora of British India;* by J. D. Hooker, C.B., &c. Part IV. pp. 240. Date of issue not given.—This commences the second volume and contains the orders *Sabiaceæ*, *Anacardiaceæ*, and *Connarceæ*, by Dr. Hooker, and the *Leguminosæ* down to the genus *Derris* (two thirds of the 132 genera), by Mr. Baker. The model of the British Colonial Floras is followed. It is curious to find our *Clitoria Mariana* as an Indian species. A. G.

2. *Compositæ Indicæ descriptæ et secus Genera Benthamii ordinatæ*, a A. B. Clarke. Calcutta, Thacker, Spink & Co. 1876. pp. 347, xlv, 8vo, 1876.—This important side-contribution to the Indian flora is in Latin, but otherwise on nearly the plan of the Flora of British India, except that the generic characters are wholly given in the conspectus; and it is supplemented by copious tables of geographical distribution. The whole appears to be very well done, and, being published at the author's own expense, the cordial thanks of botanists are justly due. A. G.

3. *Proceedings of the American Association for the Advancement of Science, 24th meeting*, 1875. Botanical Articles.—These are few, occupying only 20 pages of the thick volume, and are not of high importance. The longest is that in which Mr. Meehan asks the question "*Are Insects* [*of*] *any material aid in fertilization*" of flowers? He answers the question in the negative, but not to our satisfaction. Some of the facts are open to question, or would take in other hands a different interpretation; and several of the illustrations of supposed self-fertilization are from flowers in which other observers, with opposite prepossessions, see exquisite adaptation to crossing. As we may not ourselves rightly appreciate Mr. Meehan's ingenious argumentation (our own observations all pointing to an opposite conclusion) we state that he claims to have proved:

"First, that the great bulk of colored-flowering plants are self-fertilizers.

"Secondly, that only to a limited extent do insects aid fertilization.*

* *Apropos* to Mr. Meehan's suggestion, that, although the alpine plants of the Colorado Rocky Mountains are mostly highly colored, insects are there so rare that they can be of no material aid to fertilization, and therefore these plants must self-fertilize, it may not be amiss to introduce testimony. An entomologist now

"Thirdly, that self-fertilizers are every way as healthy and vigorous, and immensely more productive than those dependent on insect aid.

"Fourthly, that where plants are so dependent, they are the worse fitted to engage in the struggle for life."

It is not easy to perceive how the last two very comprehensive propositions are or can be demonstrated.

The next article, on *Carnivorous Plants*, by Professor Beal, of Michigan, is short and sketchy, recapitulating some facts well-known in the science, though novel to a popular assemblage; and finally, referring to *Martynia* and the vast number of small insects which are caught by its sticky glands, he suggests that "it is a true insectivorous plant." That may well be; but the observations and experiments recorded were not carried to the point of proving it, although this might not have been very difficult.

Inequilateral Leaves, by the same author, is a brief article, detailing a good number of cases, and ending abruptly with the remark: "Why these leaves have unequal lobes I cannot see; and I have no theory to offer as a probable explanation."

The Venation of a few odd Leaves, also by Professor Beal, is a short note, in which the main point is a curious suggestion as to the morphology of the odd leaf of the Ginkgo tree.

Some Observations on the structure and habits of Utricularia vulgaris, by T. B. Comstock, is an abstract merely, describing the apparatus and apparent action of the bladders. "The value of this paper is slight, except as confirmatory, in a measure, of the discoveries of others, and as an illustration of the great ease of conducting similar observations, now much needed."

Lastly, *Periodicity in Vegetation*, by James Hyatt, of Dutchess Co., N. Y., is a longer article, noting how certain plants appear and disappear in different years, in certain places, *Silene antirrhina*, for instance, abounding in 1864, 1869, and 1874, "while not a single plant has shown itself, neither in 1875, nor in any other year than those specified since 1864;" from which it would appear that "the seeds lie dormant through the intermediate period." The article concludes with some noteworthy suggestions and plans for the convenient keeping of a useful botanical diary, especially for the recording of periodical phenomena. A. G.

at my side, who has passed four summers among these mountains and made frequent visits to the alpine regions, informs me that he has "always found insects of all orders quite abundant in the Rocky Mountains, and especially so wherever flowers occur in most variety, as in the immediate vicinity of the timber-line, 10,000 to 12,000 feet," etc. Also that "insects are much more abundant everywhere in the mountains than on the plains." He has "frequently noticed the congregation of butterflies, in considerable numbers, about the bleak and barren summits of rocky peaks far above the timber-line, whither they had probably been drifted by the wind. Bees and other Hymenoptera occur in considerable variety and abundance at the timber-line. In fact, one of the best places for collecting them is among the vast fields of flowers which there occur." Finally he remarks that "although, as a rule, insects, as well as other animals, are not so plentiful in all the Rocky Mountains as in many other parts of the country, yet, comparing the alpine regions with the plains, I have always found insects *very much* more abundant in the former than in the latter." A. G.

IV. Miscellaneous Scientific Intelligence.

1. *Prof. Huxley in New York.*—Three lectures on Evolution were delivered by Prof. Huxley to a very large and interested New York audience during the week which closed with his departure for England. He appealed chiefly to American facts in his lucid and well-conducted argument, and especially to those which had been gathered by Prof. Marsh, whose "enormous collections" in the Yale Peabody Museum he had examined a few weeks before. Respecting these collections he remarked: "I can emphatically say that so far as my knowledge extends, there is nothing in any way comparable to them for extent, or for the care with which the remains have been collected, or for their scientific importance in the series of fossils." The Cretaceous birds with teeth—Marsh's Odontornithes—and also the Dinosaurs (whose tracks he had seen in large numbers during an excursion under Prof. Marsh's escort to Greenfield, Massachusetts), were referred to at length in the second lecture, and the series of horses from the American Tertiary, in the third. Prof. Huxley closed as follows: "I did not, when I commenced this series of lectures, think it necessary to preface them with a prologue, such as might be expected from a stranger and a foreigner; for during my brief stay in your country I have found it very hard to believe that a stranger could be possessed of so many friends, and almost harder to imagine that the foreigner could express himself in your language in such a way as to be so readily intelligible to all appearance; for, so far as I can judge, that most intelligent and perhaps I may add most singularly active and enterprising body of the press, your press reporters, do not seem to have been deterred by my accent from giving the fullest account of everything that I happen to have said. [Great applause.] But the vessel in which I take my departure to-morrow morning is even now ready to slip her moorings; I awake from my delusion that I am other than a stranger and a foreigner. I am ready to go back to my place and country, but before doing so, let me, by way of epilogue, tender to you my most hearty thanks for the most kind and cordial reception which you have accorded to me; and let me thank you still more for that which is the greatest compliment which can be afforded to any person in my position—the continuous and undisturbed attention which you have continued to bestow upon the long argument I have had the honor to lay before you."

2. *The Eighth Annual Report of the Department of Marine and Fisheries of Canada*, containing the Report on the Meteorological and other Observatories, for the year 1875, has recently come to hand. The Canadian office, like that of our own army, contemplates both the study of climatological statistics, and the practical utilization of meterology especially in the prognostications of the weather. Its stations are classified as the central office, ordinary stations, chief stations, reporting telegraph stations, and

publishing telegraph stations. Thirteen Canadian stations send tri-daily reports to the central office at Toronto, whence they are telegraphed to Washington. At 36 stations storm warning signals, generally as suggested by telegraph from Washington, are displayed by hoisting the storm drum; 628 warnings were issued on 55 days in the course of 1875. For those stations from which reports have been published, about 80 per cent are reported verified.

The simultaneous observations at the Canadian stations are printed in full as a part of Professor Kingston's report, followed by the monthly, quarterly and annual averages of meteorological elements and other tabular matter.

In the report of the Director of the Magnetic Observatory at Toronto, it is stated that the Kew System of self-recording apparatus has been adopted there with considerable success. At the Montreal Observatory it is stated that the anemometer by Green is furnished with a self-recording attachment as made by Hahl & Co., of Washington. At Quebec, the noon-day time-gun has been fired daily by electricity, the machinery having worked with great satisfaction. The director of this observatory, Lieutenant E. D. Ashe, hopes that the observatory will soon take a prominent place in the study of physics. At St. Johns, New Brunswick, the noon-day time-ball, which is on the Custom House building, is dropped by the director in charge of that duty.

In singular contrast with the appreciation shown by the Canadian authorities of the importance of distributing accurate time to the shipping, is the utter neglect of the subject shown in the United States, where shipmasters are obliged to obtain accurate time, before leaving port, by application to some clock-maker, whereas the Naval Observatory at Washington might easily be authorized to furnish official standard time to the whole country.

C. A.

3. *British Association.* — Extended reports of the Glasgow meeting of the British Association are published in Nature, commencing with the number for September 7th (No. 378). The address of Sir William Thomson and Mr. Wallace, forming articles in this number, have been copied from that Journal. According to a note by Mr. Wallace, later published, *Kerguelen's Land*, p 368, second line from foot, should be *Tristam d'Acunha.*

4. *Diatoms in Wheat Straw.*—The article on this subject, by Professor P. V. Wilson, in the last volume of this Journal (p. 373), has, with good reason, encountered doubts and criticisms in other journals, both at home and abroad. After its publication, when it first met the eye of our botanical editor, he wrote us at once, pronouncing the alleged facts intrinsically absurd; and, in a subsequent number of the Journal, alluded to the mistake into which he supposed the writer of the article had honestly, but ignorantly fallen. An examination of the figures of the "Forms of Diatoms found in Col. Kunker's Straw"—reproduced with severe strictures, in the American Journal of Microscopy—has led him to write us further that his explanation can apply only to a few of the figures, and that the charitable construction must be withdrawn.—Eds.

APPENDIX.

Art. XLII.—*Notice of new Tertiary Mammals.* V; by Professor O. C. Marsh.

The remains here described are from the Eocene of the Rocky Mountain region. They include a new genus of Equine mammals, allied to *Orohippus*, but an earlier and less specialized form, apparently in the direct ancestral line, and hence of much interest. All the specimens described are preserved in the Museum of Yale College.

Eohippus validus, gen. et sp. nov.

This genus is very nearly related to *Orohippus*, but may be readily distinguished from it by the dentition, the last premolar above and below being similar to the next premolar in front, and not like the adjoining true molar, as in *Orohippus*. In other respects, the teeth in the two genera are very much alike, and the dental formula is the same for both. The feet, also in their main features, are very similar, there being in each genus four well developed toes in front and three behind, but *Eohippus* has a rudiment of the outer, or fifth, metatarsal, and may have had a similar remnant of the first digit in the fore foot. The radius and ulna, and the tibia and fibula were distinct, and entire, and in most other respects the skeleton resembled that of *Orohippus*.

The present species is based mainly upon a fragmentary skeleton, with the principal teeth well preserved. These remains indicate an animal about as large as a fox, but of rather more robust proportions. Some of the more important measurements are as follows:

Extent of three lower true molars	25·4 mm.
Antero-posterior diameter of last lower molar	11·2
Transverse diameter	5·4
Antero-posterior diameter of last lower premolar	7·
Transverse diameter	5·4
Antero-posterior diameter of first upper true molar	7·
Transverse diameter	9·5

The known remains of this species are from the *Coryphodon* beds, or lowest Eocene. of New Mexico. This horizon is below that in which *Orohippus* occurs.

Eohippus pernix, sp. nov.

A smaller species of the same genus is indicated by fragmentary remains of several individuals. Most of these fossils are in excellent preservation, and among them are some of the most characteristic portions of the skeleton. The distal end of the tibia is remarkably like that of the modern horse. The astragalus, also, is quite equine in type, but the anterior portion is more elongated. It has a small facet for the cuboid, as in the horse. The molar teeth are similar in pattern to those of *Orohippus*.

The following are some of the principal measurements:

Extent of three lower true molars	20· mm.
Antero-posterior diameter of first lower true molar	6·
Transverse diameter	5·
Depth of lower jaw below first true molar	11·
Antero-posterior diameter of distal end of tibia	13·
Transverse diameter	10·
Length of astragalus	15·5
Transverse diameter in front	10·

The remains here described are from the *Coryphodon* beds, or lowest Eocene, of Wyoming.

Parahyus vagus, gen. et sp. nov.

An interesting genus of suilline mammals is represented by a nearly perfect lower jaw and a few other remains in the Yale Museum. This jaw, which has most of the teeth well preserved, shows a near affinity to *Elotherium* Pomel, and to *Helohyus* Marsh, but it may easily be separated from those genera, as it has one less premolar. It differs from the former genus, moreover, in its last lower molar, which has a well developed posterior lobe. In other respects, the teeth are very similar to those of *Elotherium*. The present genus affords an interesting example of an extinct form outside of the ancestral line which terminated in existing suillines. A similar example is seen in *Anoplotherium*.

The specimens preserved pertained to an animal about the size of a modern wild boar, but the jaws were proportionately shorter and stouter. The canine was large, and the three premolars were all compressed, and each had two roots.

The dimensions of this specimen are as follows:

Extent of six molar teeth	138· mm.
Antero-posterior diameter of last lower molar	34·
Transverse diameter	17·
Antero-posterior diameter of first true molar	19·
Transverse diameter	13·
Depth of lower jaw below first true molar	46·

The only specimens of this species now known are from the lower Eocene of Wyoming.

Dromocyon vorax, gen. et sp. nov.

A new and remarkable carnivorous mammal about the size of a large wolf is represented in the Yale Museum by a nearly complete skeleton In the form of the skull, and general character of the jaws and teeth, the genus resembles *Hyænodon*. In the present specimen there were apparently but two lower incisors in each ramus. There are seven lower molar teeth, and the last lower molar is small. The top of the skull supported an enormous sagittal crest. The brain was small and convoluted. The lower jaws are long and slender, and the condyles low.

The femur has a small third trochanter and the astragalus a facet for the cuboid. There were but four toes in front, and four behind.

Some of the more important dimensions of this skeleton are as follows:

Length of skull from occipital condyles to front of premaxillaries	280· mm.
Distance from foramen magnum to top of sagittal crest	80·
Extent of lower molar series	131·
Extent of three true molars	70·
Antero-posterior diameter of last lower molar	15·
Transverse diameter	9·
Length of third metacarpal	78·
Length of third metatarsal	88·

The remains of this species at present known, are from the Eocene of Wyoming.

Dryptodon crassus, gen. et sp. nov.

The present genus belongs in the order *Tillodontia*, and is apparently most nearly allied to *Stylinodon*. It is based upon the nearly perfect lower jaws, with most of the teeth preserved, and some fragmentary remains of the skeleton of the same individual. These specimens indicate an animal nearly as large as a Tapir. The lower jaws are very short, and massive, especially in the anterior portion. In each ramus there were ten teeth, forming a continuous series. Three of these are clearly incisors, the two anterior being small and cylindrical, and the outer one of enormous size, compressed, faced in front with enamel, and growing from a persistent pulp. Next to this, was a small tooth which may have been a canine, and immediately behind this a series of six similar molars. The latter are cylindrical, and their sides nearly or quite covered with enamel. Three are apparently premolars, and the large incisor extends beneath them.

The principal measurements of this specimen are the following:

Extent of dental series	135·	mm.
Extent of six lower molars	95·	
Antero-posterior diameter of large incisor	37·	
Transverse diameter	17·	
Antero-posterior diameter of last premolar	16·	
Transverse diameter	15·	
Depth of lower jaw below canine	80·	
Depth below first molar	60·	
Depth below last lower molar	50·	

The specimens above described are from the lower Eocene of New Mexico.

Yale College, New Haven, Oct. 23, 1876.